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THE PSYCHOLOGICAL REVIEW

MIND, MECHANISM, AND ADAPTIVE BEHAVIOR[1]

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INTRODUCTION

Since the time of Charles Darwin it has become clear not only that living organisms have gradually evolved through immense periods of time, but that man is evolution's crowning achievement. It is equally clear that man's preëminence lies in his capacity for adaptive behavior. Because of the seemingly unique and remarkable nature of adaptive behavior, it has long been customary to attribute it to the action of a special agent or substance called 'mind.' Thus 'mind' as a hypothetical entity directing and controlling adaptive behavior attains biological status possessing survival value and, consequently, a 'place in nature.' But what is this mysterious thing called mind? By what principles does it operate? Are these principles many or are they few? Are they those of the ordinary physical world or are they of the nature of spiritual essences—of an entirely different order, the non-physical?

It will, perhaps, be most economical to begin our examination of this important problem by passing briefly in review some typical phenomena of adaptive behavior which have led to the assumption of a special psychic entity. Among these may be mentioned the following: When obstacles are encountered, organisms often persist in making the same incorrect attempt over and over again; they vary their re-

[1] Presidential Address delivered before the American Psychological Association, Hanover, New Hampshire, September 4, 1936.

The author is indebted to Professor Max Wertheimer for a critical reading of this paper.

actions spontaneously; they display anticipatory reactions antedating the biological emergencies to which the reactions are adaptive; they present the phenomena of disappointment and discouragement; they strive to attain states of affairs which are biologically advantageous; they transfer to new problem situations adaptive behavior acquired in situations which, objectively considered, are totally different. The behavior of organisms is purposive in that they strive for goals or values, and in so doing manifest intelligence or insight and a high degree of individual freedom from current coercion of the environment. Whatever may be the final conclusion as to the ultimate nature of these phenomena, their biological significance in terms of survival must be immense. The task of understanding and controlling them is surely worthy of the best coöperative efforts of the biological and social sciences.

The Controversy Regarding Adaptive Behavior Is Theoretical, Not Factual

Historically, two main views have been held as to the ultimate nature of adaptive behavior. The most widely accepted of these, at the present time, is also the most ancient; its roots lie far back in primitive animism. According to this view, the principles governing adaptive behavior are essentially non-physical, mental, or psychic. The second view, despite its austerity, has received a certain amount of favor among men of science. It assumes that adaptive behavior operates ultimately according to the principles of the physical world. In our consideration of these contrasting views, it will be convenient to begin with the latter.

The physical or mechanistic view of the nature of adaptive behavior can best be stated by quoting the beautiful presentation of the raindrop analogy written by the late Albert P. Weiss:

> We may best visualize the relationship between the responses that make up the so-called purposive behavior category by the raindrop analogy. We may start with the assumption that every drop of rain in some way or other gets to the

> ocean. . . . Anthropomorphizing this condition we may say that it is the *purpose* of every drop of rain to get to the ocean. Of course, this only means that virtually every drop *does* get there eventually. . . . Falling from the cloud it may strike the leaf of a tree, and drop from one leaf to another until it reaches the ground. From here it may pass under or on the surface of the soil to a rill, then to a brook, river, and finally to the sea. Each stage, each fall from one leaf to the next, may be designated as a *means* toward the final end, the sea, . . . Human behavior is merely a complication of the same factors.[2]

The nub of Weiss's statement lies in his concluding remark that adaptive behavior is merely a 'complication' of the same factors as those which are involved in the behavior of a drop of water finding its way from an inland cloud to the sea. Obviously, Weiss did not mean to say that the several forms of seeking and striving behavior characteristic of the higher organisms are brought about by the various compoundings of such processes as evaporation, condensation, splashing, and flowing. The context of the quotation shows that he meant that ultimately the complex forms of purposive behavior would be found to derive from the same *source* as those from which the raindrop phenomena are derived; *i.e.*, from the basic entities of theoretical physics, such as electrons and protons. He discusses these latter concepts explicitly and at length.

Passing to the more orthodox view, that adaptive behavior is essentially non-physical, or psychic, the words of A. S. Eddington may be taken as a point of departure. In his book, 'The nature of the physical world,'[3] Eddington remarks:

> Conceivably we might reach a human machine interacting by reflexes with its environment; but we cannot reach rational man morally responsible. [P. 343.] . . . In a world of æther and electrons we might perhaps encounter *nonsense;* we could not encounter *damned nonsense.*

[2] Albert P. Weiss, A theoretical basis of human behavior, Columbus, Ohio: R. G. Adams and Company, 1925, pp. 346–347.

[3] New York: The Macmillan Company, 1929, p. 345.

The significance of Eddington's statement centers around the word *reach*. From the present point of view, he seems to be saying that we cannot reach the highest forms of adaptive behavior, such as complex problem solution (rational behavior) and certain complex forms of social behavior involving the implicit verbal coercion of the behavior of the individual (moral behavior) if we start out merely with æther and electrons; we must begin with something non-physical, or psychic—presumably consciousness.

Thus the issue is joined. We are presented with the paradox of Eddington, the physicist, apparently insisting that the higher forms of behavior are at bottom non-physical, whereas Weiss, the psychologist, insists that they are fundamentally non-phychological!

But what, exactly, is the issue? Is it, for example, a difference as to an ordinary matter of observed fact? Do Eddington and those who share his view claim to have made certain observations which are in conflict with a corresponding set of observations supposed to have been made by Weiss and those with a mechanistic leaning? The dispute involves nothing of this nature. It is clear that the controversy is definitely a theoretical one. Eddington seems to be implying that we *can not* reach a sound theory of rational, purposive and moral behavior if we set out with nothing but æther and electrons. Weiss is saying, by implication, that a sound theory of such behavior *can* be reached by setting out with nothing but electrons and protons.

The Methodology of Scientific Theory Differentiated from That of Philosophical Speculation

Having located definitely in the field of theory the contrasted views represented in a general way by Weiss and Eddington, we face at once the critical question of whether the problem lies within the range of the operation of scientific methodology. If it does, what is that methodology? How is it to be applied to the question before us in a way which will avoid the interminable wrangles and philosophical futilities so long associated with the mind-body problem? It

will be necessary to go into the matter of methodology rather thoroughly, in part because of its central importance for our present problem, but in part also because of the widespread misconceptions regarding it due to our early associations with philosophy. With the question of methodology clarified we shall return to Weiss and Eddington in the hope of demonstrating its concrete application.

The essential characteristics of a sound scientific theoretical system, as contrasted with ordinary philosophical speculation, may be briefly summarized under three heads:

1. A satisfactory scientific theory should begin with a set of explicitly stated postulates accompanied by specific or 'operational' definitions of the critical terms employed.

2. From these postulates there should be deduced by the most rigorous logic possible under the circumstances, a series of interlocking theorems covering the major concrete phenomena of the field in question.

3. The statements in the theorems should agree in detail with the observationally known facts of the discipline under consideration. If the theorems agree with the observed facts, the system is probably true; if they disagree, the system is false. If it is impossible to tell whether the theorems of a system agree with the facts or not, the system is neither true nor false; scientifically considered, it is meaningless.

Since concrete example is more illuminating and more convincing than abstract statement, there is reproduced below a small scientific theoretical system in which an attempt has been made to conform to the above principles. There may be found (p. 15 ff.) a number of definitions, which are followed (p. 16 ff.) by six postulates. The system concludes with a series of thirteen theorems (p. 17 ff.), each derived from the postulates by a process of reasoning analogous to that ordinarily employed in geometry.

At first sight the formal characteristics of scientific theory look very much like those of philosophical speculation and even of ordinary argumentation, from which philosophical speculation can scarcely be distinguished. At their best, both scientific theory and philosophical speculation set out

from explicit postulates; both have definitions of critical terms; both have interlocking theorems derived by meticulous logic. Consider, for example, Spinoza's 'Ethic,' a philosophical work of the better sort. This has all of the above characteristics in almost exactly the same form as the miniature scientific system which is presented below. Where, then, lie the great difference and superiority of the scientific procedure?

The answer, while extending into many complex details, rests upon a single fundamental principle. The difference is that *in philosophical speculation there is no possibility of comparing a theorem with the results of direct observation.* An obvious example of this impossibility is seen in Spinoza's famous pantheistic theorem, Proposition XIV, from Part One of his 'Ethic':

Besides God no substance can be, nor can be conceived.

It is difficult to imagine subjecting such a theorem as that to an observational test.

Consider, by way of contrast, a really scientific procedure, one carried out by Galileo at about the same time that Spinoza was writing. The Copernican hypothesis concerning the nature of the solar system was then in violent dispute. From this hypothesis, together with a few familiar principles concerning the behavior of light, it follows logically as a theorem that the planet Venus, like the moon, should show the crescent and all the other stages between the full and dark phases. Presumably led by this deduction, Galileo, with a telescope of his own construction, made the necessary observations on Venus and found the phases exactly as demanded by the theorem. Here we have the indispensable observational check demanded by science but lacking in philosophy.

But why, it will be asked, is it so imperative to have an observational check on the theorems of a system if the system is to merit serious consideration by scientists? To answer this question adequately it will be necessary to consider in a little detail the characteristics of postulates, the procedure in selecting them, and the methodology of their substantiation.

It is important to note at the outset that in scientific theory postulates tend to be of two kinds. First, there are postulates which are mere matters of fact; *i.e.*, they are matters of relatively simple and direct observation. Second, there are postulates which by their nature cannot conceivably be matters of direct observation. The classical investigation of Galileo just considered contains examples of both types. The principles of light and shadow upon which lunar and planetary phases depend are obviously matters of ordinary, everyday, direct terrestrial observation, and so represent postulates of the first type. On the other hand, the Copernican hypothesis as to the relative movements of the several components of the solar system is not susceptible to direct observation, and so represents postulates of the second type.

In scientific theory, owing to the continuous checking of theorems arrived at deductively against the results of direct observation, both types of postulates are constantly receiving *indirect* verification or refutation. Thus postulates capable of the direct approach are susceptible of two independent kinds of test, the direct and the indirect. But the continuous indirect test is of special importance for the postulates incapable of the direct approach. Were it not for this they would be subject to no observational verification at all, and scientific theory would in this respect have no more safeguard against erroneous basic assumptions than has philosphical speculation. Thus Galileo's brilliant observations of the phases of Venus not only gave the scientific world some new facts but, of far greater importance, they substantiated in a convincing, though indirect, manner the fundamental Copernican hypothesis.[4]

[4] Many persons have been puzzled by the paradox that in science a deduction frequently sets out with postulates which are by no means securely established, whereas in ordinary argumentation there is the greatest insistence upon the certainty of the premises upon which the argument is based. The explanation of this paradox lies largely in the difference of objective in the two cases. Argument ordinarily seeks to convince by a deductive procedure of something which under the circumstances is not directly observable; otherwise there would be no point in performing the deduction. It is clear that if the person to whom the argument is directed does not agree with the premises he will not agree with the conclusion and the whole procedure will

Whenever a theorem fails to check with the relevant facts, the postulates which gave rise to it must be ruthlessly revised until agreement is reached. If agreement cannot be attained, the system must be abandoned. In this constant revision there is a definite tendency to choose and formulate the postulates in a way which will make them yield the deductions desired. Such a procedure involves an obvious element of circularity. This is particularly the case where the system is small and where the postulates are purely symbolic constructs or inventions and therefore not subject to direct investigation. Even so, the choice of postulates to fit the facts is methodologically legitimate and, upon the whole, desirable. One important reason for this is that a postulate or hypothesis so arrived at may lead to a *direct*, experimental confirmation in case it is capable of the direct approach.[5] In such an event, of course, all circularity disappears.

But if the system is truly scientific in nature, the circularity just considered is only a temporary phase even when one or more of the postulates are insusceptible to direct investigation. It is precisely in this connection that scientific method shows its incomparable superiority over philosophical speculation. A sound set of postulates should lead to the deduction of theorems representing phenomena never previously investigated quite as logically as of theorems representing phenomena already known when the postulates were formulated. When a theorem representing novel phenomena receives direct observational confirmation there is no possi-

be futile. In science, on the other hand, the situation may be almost completely reversed; the conclusion (or theorem) may be known observationally at the outset, but the premises (or postulates) may at first be little more than conjectures and the logical process quite circular. For the methodology of resolving this circularity, see p. 8 ff.

[5] From the experimental point of view the process of developing systematic theory thus leads in two directions. On one hand it leads to the investigation of theorems derived from postulates of the system, and on the other to the direct investigation of postulates which appear to be required as assumptions for the deductive explanation of facts already known. Since phenomena of the latter type are fundamental in a strict sense, their investigation is of the highest significance. A background of systematic theory thus often directly suggests fundamental investigations which might be indefinitely delayed under the usual procedure of random, and even of systematic, exploration.

bility of circularity; as a consequence the probability that the postulates directly involved are sound is very definitely increased.[6] Thus the fact that Venus shows lunar phases could not have been known to Copernicus when he formulated his epoch-making hypothesis, because the telescope had not yet been invented. Accordingly their discovery by Galileo constituted strong positive evidence of the essential soundness of the Copernican hypothesis regarded as a postulate. This classical example of the observational but indirect confirmation of the soundness of postulates will serve as a fitting conclusion for our general consideration of theoretical methodology.

The Recognized Scientific Methodology Has Not Been Applied to the Behavior Controversy

We turn now to the question of whether the recognized scientific methodology is really applicable to a resolution of the controversy concerning the basic nature of adaptive behavior. At first glance the prospect is reassuring. It becomes quite clear, for example, what Weiss and Eddington should have done to substantiate their claims. They should have exhibited, as strict logical deductions from explicitly stated postulates, a series of theorems corresponding in detail to the concrete manifestations of the higher forms of human behavior. Then, and only then, they might proceed to the examination of the postulates of such system. To substantiate his position Weiss would have to show that these postulates concern essentially the behavior of electrons, protons, etc.; and Eddington to support his assertions would need

[6] A single unequivocal disagreement between a theorem and observed fact is sufficient to assure the incorrectness of at least one of the postulates involved. But even if the postulates of a system generate a very long series of theorems which are subsequently confirmed without exception, each new confirmation merely adds to the *probability* of the truth of such postulates as are incapable of direct observational test. Apparently this indirect evidence never reaches the crisp certainty of a deductive conclusion in which the postulates are directly established, except in the highly improbable situation where all the possible deductions involving a given postulate have been tested with positive results. According to the theory of chance, the larger the sample from this possible total which has been tried and found without exception to be positive, the greater the probability that a new deduction based on the same set of postulates will be confirmed when tested.

to show that the postulates of a successful system are primarily phenomena of consciousness.[7] The formal application of the methodology is thus quite clear and specific.

But here we meet an amazing paradox. In spite of the calm assurance of Weiss as to the truth of his statement that purposive behavior is at bottom physical, we find that he neither presents nor cites such a system. Indeed, he seems to be quite oblivious of such a necessity. Turning to Eddington, we find exactly the same paradoxical situation. Notwithstanding his positive, even emphatic, implications that moral behavior must be conscious or psychic in its ultimate nature, we find him neither presenting nor citing a theoretical system of any kind, much less one derived from psychic or conscious postulates. This paradox is particularly astonishing in the case of Eddington because he has been active in the field of physical theory and should, therefore, be sophisticated regarding the essential methodology involved in scientific theory in general. Surely the same logic which demands strict deduction from explicitly stated postulates in physical theory demands it for the theory of adaptive and moral behavior. And surely if we demand it of a mechanistic theory of the more recondite forms of human behavior, as Eddington seems emphatically to do, there is no hocus-pocus whereby a psychic view of such behavior may be maintained without the same substantial foundation.

A Demonstration of the Application of Theoretical Methodology to Adaptive Behavior

But if neither Weiss nor Eddington, nor any other writer in this field, has been able to bring forward the indispensable systematic theory as a prerequisite of the logical right to express a valid conclusion concerning the ultimate nature of higher adaptive behavior, may this not mean that the attainment of such a system is impossible, and that, consequently,

[7] It is here assumed as highly probable that if the two approaches are strictly in conflict, only one would be successful. In the course of the development of scientific theoretical systems, however, it is to be expected that during the early stages several different systems may present appreciable evidences of success. See The conflicting psychologies of learning—A way out, Psychol. Rev., 42, 1936; especially pp. 514–515.

the problem still remains in the realm of philosophical speculation? There is reason to believe that this is not the case. The ground for optimism lies in part in the small theoretical system which is presented below (p. 15).

By way of introduction to the system we may begin with the consideration of Theorem I (p. 17). In brief, this theorem purports to show that Pavlov's conditioned reactions and the stimulus-response 'bonds' resulting from Thorndike's so-called 'law of effect' are in reality special cases of the operation of a single set of principles. The major principle involved is given in Postulate 2. Briefly, this postulate states the assumption of the present system concerning the conditions under which stimuli and reactions become associated. The difference in the two types of reaction thus turns out to depend merely upon the accidental factor of the temporal relationships of the stimuli to the reactions in the learning situation, coupled with the implication that R_G, which in part serves to mark a reinforcing state of affairs, is also susceptible of being associated with a new stimulus.[8] The automatic, stimulus-response approach thus exemplified is characteristic of the remainder of the system.

A consideration of Theorem II will serve still further as an orientation to the system before us. We find this theorem stating that both *correct* and *incorrect* reaction tendencies may be set up by the conditioning or associative process just referred to. Our chief interest in this theorem, as an introduction to the system, concerns the question of whether the terms 'correct' and 'incorrect' can have any meaning when they refer to reaction tendencies which are the result of a purely automatic process of association such as that presented by Postulate 2. It is believed that they have a very definite meaning. Definitions 7 and 8 state in effect that correctness or incorrectness is determined by whether the reaction tendency under given conditions is, or is not, subject to experimental extinction. Such purely objective or behavioral

[8] In effect this deduction purports to show that the Pavlovian conditioned reflex is a special case under Thorndike's 'law of effect,' though Thorndike might not recognize his favorite principle as formulated in Postulate 2. For a fuller but less formal discussion of this point see *Psychol. Bull.*, 1935, 32, 817–822.

definitions of numerous terms commonly thought of as applying exclusively to experience, as distinguished from action, are characteristic of the entire system.

With this general orientation we may proceed to the theorems more specifically concerned with adaptive behavior. The proof of the first of these, Theorem III, shows that under certain circumstances organisms will repeatedly and successively make the same incorrect reaction. At first sight this may seem like a most commonplace outcome. However, when considered in the light of the definition of correctness given above it is evident that this theorem differs radically from what might be deduced concerning the behavior of a raindrop or a pebble moving in a gravitational field.[9]

Theorem IV states that after making one or more incorrect reactions an organism will spontaneously vary the response even though the environmental situation remains unchanged. This theorem is noteworthy because it represents the classical case of a form of spontaneity widely assumed, as far back as the Middle Ages, to be inconceivable without presupposing consciousness.

Theorem V states that when an organism originally has both correct and incorrect excitatory tendencies evoked by a single stimulus situation, the correct tendency will at length be automatically selected in preference to stronger incorrect ones.[10] This theorem, also, has been widely regarded as impossible of derivation without the presupposition of consciousness. Otherwise (so it has been argued) how can the organism know which reaction to choose?

[9] It may be suggested that if water should fall into a hollow cavity on its way to the sea, it might at first oscillate back and forth vigorously and then gradually subside, each oscillation corresponding to an unsuccessful attempt and the gradual cessation, to experimental extinction. In all such cases the discussion as to whether the observed parallelism in behavior represents an essential similarity or a mere superficial analogy requires that both phenomena possess a thorough theoretical basis. *If the two phenomena are deducible from the same postulates and by identical processes of reasoning, they may be regarded as essentially the same, otherwise not.* But if one or both lacks a theoretical basis such a comparison cannot be made and decision can ordinarily not be reached. Much futile argument could be avoided if this principle were generally recognized.

[10] See Simple trial-and-error learning: A study in psychological theory, PSYCHOL. REV., 1930, 37, 241–256; especially pp. 243–250.

Theorem VI represents the deduction that in certain situations the organism will give up seeking, *i.e.*, cease making attempts, and thus fail to perform the correct reaction even when it possesses in its repertoire a perfectly correct excitatory tendency. The substance of this proof lies in the expectation that the extinction resulting from repeated false reactions will cause indirectly a critical weakening of a non-dominant but correct reaction tendency. This theorem is of unusual importance because it represents the deduction of a phenomenon not as yet subjected to experiment. As such it should have special significance as a test of the soundness of the postulates.

With Theorems VII and VIII we turn to the problem of anticipatory or preparatory reactions. The proof of Theorem VII derives, from the principles of the stimulus trace and conditioning (Postulates 1 and 2), the phenomenon of the antedating reaction. The substance of this theorem is that after acquisition, learned reactions tend to appear in advance of the point in the original sequence at which they occurred during the conditioning process.[11] Pursuing this line of reasoning, Theorem VIII shows that in the case of situations demanding flight, such antedating reactions become truly anticipatory or preparatory in the sense of being biologically adaptive to situations which are impending but not yet actual. Thus we arrive at behavioral foresight, a phenomenon evidently of very considerable survival significance in animal life and one frequently regarded as eminently psychic, and inconceivable without consciousness.[12]

Passing over Theorem IX, which lays some necessary groundwork, we come to Theorem X. Here we find a deduction of the existence of the fractional anticipatory goal reaction. Of far greater significance from our present point of view, the deduction purports to show that through the action of mere association the fractional anticipatory reaction tends automatically to bring about on later occasions the

[11] See A functional interpretation of the conditioned reflex, PSYCHOL. REV., 1929, 36, 498–511; especially pp. 507–508.

[12] See Knowledge and purpose as habit mechanisms, PSYCHOL. REV., 1930, 37, 511–525; especially pp. 514–516.

state of affairs which acted as its reinforcing agent when it was originally set up. For this and other reasons it is believed that the anticipatory goal reaction is the physical basis of expectation, of intent, of purpose, and of guiding ideas.[13]

Theorem XI represents a deduction of the phenomenon of behavioral disappointment [14] as manifested, for example, by Tinklepaugh's monkeys. When these animals had solved a problem with the expectation of one kind of food they would tend to refuse a different kind of food, otherwise acceptable, which had been surreptitiously substituted.[15]

Theorem XII purports to be the deduction of the principle that organisms will strive actively to attain situations or states of affairs which previously have proved to be reinforcing. The automaticity deduced in the proof of Theorem X has here reached a still higher level. This is the capacity to surmount obstacles. But with the ability to attain ends in spite of obstacles comes automatically a genuine freedom (Definition 18), of great biological value but in no way incompatible with determinism.[16]

Theorem XIII is also derived with the aid of the fractional anticipatory goal reaction. This theorem represents the phenomenon of the adaptive but automatic transfer of learned reactions to situations having, as regards *external* characteristics, nothing whatever in common with the situations in which the habits were originally acquired. This, once more, is a form of adaptive behavior of the greatest survival significance to the organism, and one supposed in certain quarters to be impossible of derivation from associative principles.

[13] See Goal attraction and directing ideas conceived as habit phenomena, PSYCHOL. REV., 1931, 38, 487–506.

[14] It is to be observed from a comparison of Definitions 9 and 16 that *Disappointment* necessarily presupposes a specific expectation or intent (r_G), whereas *Discouragement* does not.

[15] O. L. Tinklepaugh, An experimental study of representative factors in monkeys, *J. Comp. Psychol.*, 1928, 8, 197–236. See especially p. 224 ff.

[16] An additional element of interest in this theorem is the fact that the fundamental phenomenon of motivation seems to have been derived from the ordinary principle of association (Postulate 2). If this deduction should prove to be sound, it will have reduced the two basic categories of motivation and learning to one, the latter being primary.

This is believed to be a low but genuine form of insight and a fairly high order of the 'psychic.'

This concludes the list of formally derived theorems. They have been selected from a series of fifty or so which are concerned with the same subject. None of these theorems 'reaches' Eddington's 'rational man morally responsible.' They accordingly are not offered as a basis for deciding the ultimate nature of such behavior. They *are* offered as a concrete and relevant illustration of the first and most essential step in the methodology which must be followed by Eddington, or anyone else who would determine the basic nature of the higher forms of behavior. Incidentally they are offered as specific evidence that such problems, long regarded as the peculiar domain of philosophy, are now susceptible of attack by a strictly orthodox scientific methodology.

Adaptive Behavior—A Scientific Theoretical System in Miniature[17]

Definitions

1. A *reinforcing state of affairs* (Postulate 3) is one which acts to give to the stimulus-trace component (Postulate 1) of preceding or following temporal coincidences consisting of a stimulus trace and a reaction, the capacity to evoke the reaction in question (Postulate 2).

2. *Experimental extinction* is the weakening of a conditioned excitatory tendency resulting from frustration or the failure of reinforcement (Postulate 4).

3. *Frustration* is said to occur when the situation is such that the reaction customarily evoked by a stimulus complex cannot take place (Postulate 4).

4. *Seeking* is that behavior of organisms in trial-and-error situations which, upon frustration, is characterized by varied alternative acts all operative under the influence of a common drive (S_D).

5. An *attempt* is a segment of behavior the termination of which is marked by either reinforcement or extinction.

6. A *simple trial-and-error situation* is one which presents to an organism a stimulus complex which tends to give rise to multiple reaction tendencies which are mutually incompatible, one or more of them being susceptible to reinforcement and one or more of them not being so susceptible.

7. A *correct* or 'right' reaction is a behavior sequence which results in reinforcement.

8. An *incorrect* or 'wrong' reaction is a behavior sequence which results in experimental extinction.

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[17] The author is greatly indebted to Dr. E. H. Rodnick and Mr. D. G. Ellson for detailed criticisms and suggestions during the original preparation of the system which follows. Thanks are also due Professor K. F. Muenzinger, Dr. R. T. Ross, and Dr. R. K. White for criticisms given since the presentation at Hanover.

9. *Discouragement* is the diminution in the power of one excitatory tendency to evoke its normal reaction, this diminution resulting from one or more unsuccessful attempts involving a second reaction.

10. A behavior sequence is said to be *directed* to the attainment of a particular state of affairs when there appears throughout the sequence a characteristic component (r_G) of the action (R_G) closely associated with the state of affairs in question and this component action (r_G) as a stimulus tends to evoke an action sequence leading to the total reaction (R_G) of which the component constitutes a part.

11. Striving is that behavior of organisms which, upon frustration, displays varied alternative action sequences, all *directed* by an intent (r_G) to the attainment of the same reinforcing state of affairs.

12. A *goal* is the reinforcing state of affairs towards the attainment of which a behavior sequence of an organism may be directed by its intent (r_G).

13. An organism is said to *anticipate* a state of affairs when there is active throughout the behavior sequence leading to the state of affairs a fractional component (r_G) of the action associated with the state of affairs in question.

14. *Success* is the culmination of striving which is characterized by the occurrence of the full reaction (R_G) of which the fractional anticipatory component (r_G) is a part.

15. *Failure* is the culmination of striving which is characterized by the lack of the enactment of the full reaction (R_G) of which the fractional component (r_G) is a part.

16. *Disappointment* is the diminution in the power of one reinforcing situation to evoke appropriate consummatory reaction, this diminution (Postulate 4) resulting from the failure of a second reaction sequence directed (by an intent, or r_G) to a different reinforcing situation from that to which the first was directed, both being based on the same drive (S_D).

17. A *habit-family hierarchy* consists of a number of habitual behavior sequences having in common the initial stimulus situation and the final reinforcing state of affairs.

18. *Individual freedom* of behavior, so far as it exists, consists in the absence of external restraint.

Postulates

1. The adequate stimulation of a sense organ initiates within the organism a neural reverberation which persists for some time after the stimulus has ceased to act, the absolute amount of the reverberation diminishing progressively to zero but at a progressively slower rate. (Stimulus trace.)

2. When a reaction and a given segment of a stimulus-trace (Postulate 1) repeatedly occur simultaneously and this coincidence occurs during the action of a drive (S_D) and temporally close to a reinforcing state of affairs (Definition 1), this and stronger segments of the stimulus trace tend progressively to acquire the capacity to evoke the reaction, the strength of the association thus acquired manifesting a negatively accelerated diminution with distance of the associates from the reinforcing state of affairs. (Positive association.)

3. A characteristic stimulus-reaction combination (S_G — — → R_G) always marks reinforcing states of affairs (Definition 1). The particular stimulus-response combination marking the reinforcing state of affairs in the case of specific drives is determined empirically, *i.e.*, by observation and experiment. (Mark of reinforcing state of affairs.)

4. When a stimulus evokes a conditioned (associative) reaction (Postulate 2) and this event does not occur within the range of the reinforcing state of affairs (Defi-

nition 1 and Postulate 3), or when an excitatory tendency in a behavior sequence encounters a situation which makes the execution of the act impossible (Definition 3), the excitatory tendency in question undergoes a diminution in strength with a limit below the reaction threshold (Definition 2), this diminution extending in considerable part to other excitatory tendencies which may be operative at the same time or for some time thereafter. (Negative association or experimental extinction.)

5. The strength of any given increment of either positive or negative association (Postulates 2 and 3) diminishes with the passage of time, and the portion remaining shows a progressively greater resistance to disintegration with the increase in time since its acquisition, a certain proportion of each increment being permanent. (Negative retention or forgetting.)

6. Each reaction of an organism gives rise to a more or less characteristic internal stimulus. (Internal stimulation.)

Key to Diagrams

S = an adequate stimulus together with the resulting trace (Postulate 1).
S_D = the stimulus associated with a drive, such as hunger.
S_G = the stimulus associated with the goal or reinforcing state of affairs.
s = an internal stimulus resulting from a reaction.
R = a reaction.
R_G = the reaction associated with the goal or reinforcing state of affairs.
r_G = a fractional component of the goal reaction.
— — → = excitatory tendency from stimulus to reaction.

⇝ = causal connection of a non-stimulus-reaction nature.

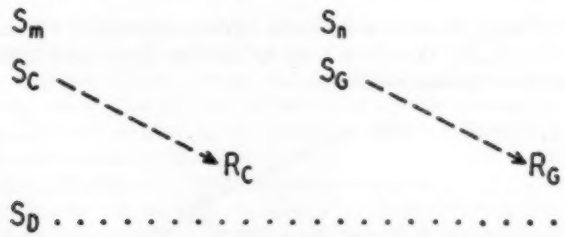
. . . = a continuation or persistence of a process, as of a drive (S_D). Distance from left to right represents the passage of time.

Theorems

I

The Pavlovian conditioned reaction and the Thorndikian associative reaction are special cases of the operation of the same principles of learning.

1. Suppose that in the neighborhood of a sensitive organism stimuli S_C and S_G occur in close succession, that these stimuli in conjunction with the drive (S_D) evoke reactions R_C and R_G respectively, that S_m coincides in time with S_C while S_n coincides in time with S_G, and that (Postulate 1) the stimulus trace of S_m extends to R_C, and the stimulus trace of S_n extends to R_G.



2. From (1) and Postulate 1, it follows that one phase of the stimulus trace of S_m will coincide with R_C and one phase of the stimulus trace of S_n will coincide with R_G.

3. Now, by Postulate 3, the combination S_G — — → R_G marks a reinforcing state of affairs.

4. From (1), (2), (3), and Postulate 2 it follows, among other things, that the trace of S_n will become conditioned to R_G, and the trace of S_m will be conditioned to R_C, yielding the following excitatory tendencies:

$$S_m \text{ — — → } R_C$$
$$S_n \text{ — — → } R_G$$

5. But by (3) and (4) the reaction of the newly acquired excitatory tendency S_n — — → R_G is that intimately associated with the reinforcing state of affairs, which identifies it as a conditioned reaction of the Pavlovian type.

6. On the other hand, by (3) and (4) the reaction of the excitatory tendency S_m — — → R_C is a reaction distinct from that of the reinforcing state of affairs, which identifies it as an associative reaction of the Thorndikian type.

7. By (5) and (6) both the Pavlovian and the Thorndikian types of reaction have been derived from (1), (2), (3), and (4) jointly, and these in turn from the same principles of learning (Postulates 1, 2, and 3).

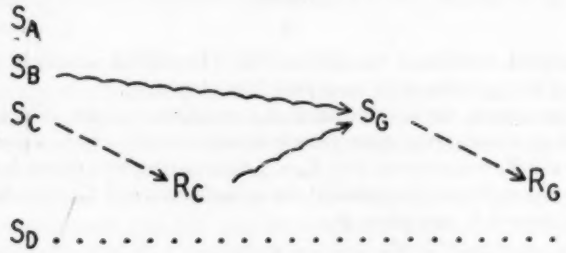
8. From (7) the theorem follows.

Q. E. D.

II

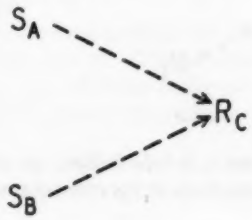
Both correct (right) and incorrect (wrong) reactions may be set up by the conditioning (associative) process.

1. Let it be supposed that an organism capable of acquiring associative reactions (Postulate 2) is, a number of times, stimulated simultaneously by S_A, S_B, S_C, and S_D; that S_G evokes reaction R_C; that the stimulus trace (Postulate 1) of S_A and S_B extend as far as R_C; that the object represented by S_B, in conjunction with act R_C, produces (causes) in the external world the event yielding the stimulus S_G; and finally that S_G evokes R_G.



2. By Postulate 3, S_G — — → R_G marks a reinforcing state of affairs.

3. From (1), (2), and Postulates 1 and 2, it follows that among other associative tendencies the following must be set up:



4. Now suppose that at a later time, S_B *alone* should evoke R_C. It follows from (1) that S_B, in conjunction with R_C, will cause to occur the event in the external world which will yield the stimulus S_G which, in turn, will evoke R_G.

5. But, once more, by Postulate 3, S_G — — → R_G marks a reinforcing state of affairs from which it follows that under the special new conditions of (4) the reaction tendency S_B — — → R_C will still be reinforced.

6. From (5) and Definition 7 it follows that S_B — — → R_C must be a correct or 'right' reaction.

7. Let us suppose, on the other hand, that S_A *alone* should evoke R_C. It follows from (1) that the external event giving rise to S_G will not occur (S_B being absent), and the excitatory tendency S_A — — → R_C will not be reinforced and, by Postulate 4, will suffer experimental extinction.

8. From (7) and Definition 8 it follows that S_A — — → R_C will be an incorrect or 'wrong' reaction.

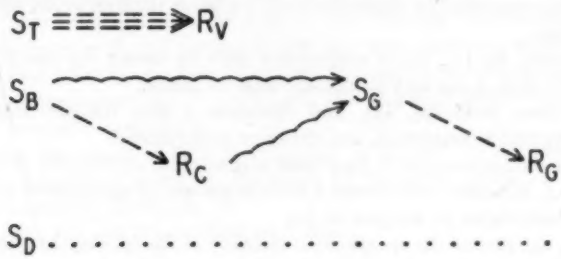
9. From (6) and (8) the theorem follows.

Q. E. D.

III

Simple trial-and-error situations may arise in which the organism will make repeated incorrect reactions.

1. Let it be supposed that we have the simultaneous stimulus situation $S_T S_B S_D$ with the component S_B (step 3, Theorem II) evoking R_C; that S_B and R_C when operating jointly cause S_G, S_G evoking R_G, whereas S_T evokes R_V with an excitatory tendency exceeding that of S_B to R_C by an amount greater than the weakening effect (Postulate 4) of several unreinforced attempts (Definition 5); that R_V is not followed by its usual reinforcing sequence (S'_G — — → R'_G); and that the external stimulus situation after each attempt becomes exactly the same as before.



2. From (1) it follows that reaction R_V will take place at once after the organism encounters the compound stimulus $S_T S_B S_D$.

3. Now, by (1) the situation is such that R_V cannot be followed by its accustomed reinforcing sequence, so that this behavior sequence must be interrupted.

4. From (3) and Postulate 4, the excitatory tendency from S_T to R_V will be weakened by experimental extinction.

5. By (4) and Definition 8, R_V is an incorrect reaction.

6. By (1) and (2) the stimulus situation after the first R_V reaction must be the same as at the beginning, and the excitatory tendency to R_V must still be considerably in excess of that to R_C, from which it follows that R_V will occur a second time, and so on.

7. But, by (2), we have a first reaction (R_V), which, by (5) is incorrect and by (6) we have a repetition of this incorrect reaction, from which the theorem follows.

Q. E. D.

IV

Organisms in simple trial-and-error situations may manifest spontaneous variability of reaction, the objective situation remaining constant.

1. Suppose the situation in (1) of Theorem III with the additional assumption that excitatory tendency $S_B \text{ — — → } R_C$ shall be strong enough to resist all generalized inhibitory effects (Postulate 4) sufficiently to escape becoming subliminal.

2. By (1) and Theorem III, it follows that reaction R_V will take place repeatedly.

3. By (1) and (2), reaction $S_T \text{ — — → } R_V$ will not be followed by reinforcement (neither $S'_G \text{ — — → } R'_G$ nor $S_G \text{ — — → } R_G$), which failure (Postulate 4) will progressively weaken the tendency to R_V.

4. From (1) and (3) it follows that the reaction tendency to R_V must finally become weaker than that to R_C, at which point the stimulus complex $S_T S_B S_D$ will evoke reaction R_C.

5. But the shift from reaction R_V (2) to R_C (4) constitutes a variability of reaction.

6. Meanwhile, by (1) the objective situation has not changed.

7. From (5) and (6) the theorem follows.

Q. E. D.

V

Organisms in simple trial-and-error situations beginning with erroneous reactions may, after a sufficiently large number of attempts, come to give an indefinitely long series of successive correct reactions.

1. Let us assume the situation in step (1) of the deduction of Theorem IV.

2. By (1) and steps (2), (3), and (4) of Theorem IV, reaction tendency R_V will be progressively weakened by extinction until it is below the level of R_C, when the latter will take place.

3. Moreover, by (1), R_C in conjunction with S_B causes S_G; and S_G evokes R_G which, by Postulate 3, marks a reinforcing state of affairs.

4. It follows from (2), (3), and Postulate 2 that the excitatory tendency $S_B \text{ — — → } R_C$ will be reinforced, and therefore strengthened.

5. But a certain amount of time must elapse while reaction R_C is taking place; by Postulate 5, this time must permit a certain amount of spontaneous recovery from experimental extinction on the part of R_V.

6. Now, the rate of the spontaneous recovery of R_V (5) may be either (*A*) more rapid than the gain in strength of R_C through the latter's reinforcement, or (*B*) it may be less rapid, or (*C*) the two processes may take place at the same rate. If it is less rapid, or if the two processes take place at the same rate, R_C will maintain its dominance, thus giving an indefinitely long series of correct reactions (Definition 7); from which the theorem follows.

7. But suppose, on the other hand, that the rate of the spontaneous recovery of R_V from its experimental extinction is faster than the gain in strength of R_C through its reinforcement (6). It follows that on this alternative R_V must again become dominant.

8. From (7) it follows by reasoning analogous to that in (2) that R_V will occur repeatedly until depressed by further experimental extinction below the strength of R_C when the latter will again occur, to be further reinforced, and so on.

9. Now it follows from (4) and (8) together with Postulate 5, that after each complete cycle of reversal of R_V and R_C, the former will retain a certain amount of its weakening which will not yield to spontaneous recovery and the latter will retain a certain amount of the strengthening which will not yield to forgetting.

10. It follows from (9) that if the cyclical alternation were to go on indefinitely, the tendency to R_V must be weakened to zero and that to R_C must be strengthened to its maximum.

11. It is evident from (10) that at some point in the progressive shift in the basic strengths of R_V and R_C the two movements must cross, at which point R_C will be permanently dominant over R_V irrespective of spontaneous recovery or forgetting, and there will then follow an indefinitely long series of successive correct reactions.

12. From (6) and (11) the theorem follows.

Q. E. D.

VI

In simple trial-and-error learning situations, failure of final correct reaction will, under certain conditions, result from discontinued effort.

1. Suppose the situation in (1) of Theorem III except that the excitatory tendency $S_B \text{— — →} R_C$ is at the outset only a little above the reaction threshold.

2. From (1) and Theorem III, false reaction R_V will be made repeatedly.

3. By (1) and (2), reaction tendency $S_T \text{— — →} R_V$ will not be followed by reinforcement, which failure (by Postulate 4) will, if not interrupted, gradually weaken $S_T \text{— — →} R_V$ to zero.

4. By (3) and Postulate 4, the weakening of $S_T \text{— — →} R_V$ will extend in considerable part to $S_B \text{— — →} R_C$.

5. Now, by (1) the super-threshold margin of strength of $S_B \text{— — →} R_C$ may be smaller than any assigned finite value, from which it follows that it may be smaller than the depressing effects (4) arising from the extinction of $S_T \text{— — →} R_V$.

6. It follows from (5) that before $S_T \text{— — →} R_V$ may be extinguished beneath the level of $S_B \text{— — →} R_C$ the latter will also have been depressed below the reaction threshold so that when $S_T \text{— — →} R_V$ reaches zero and ceases action, the potentially correct reaction tendency, $S_B \text{— — →} R_C$, will also be unable to function even though without any competition whatever.

7. But the depression of both the tendency to R_V and R_C as shown in (6) will bring about a cessation of attempts (Definition 5), the latter of which (1) would have been a correct reaction (Definition 7).

8. From (3), (4), and (7) the theorem follows.

Q. E. D.

Corollary 1.

Organisms capable of acquiring competing excitatory tendencies will manifest discouragement.

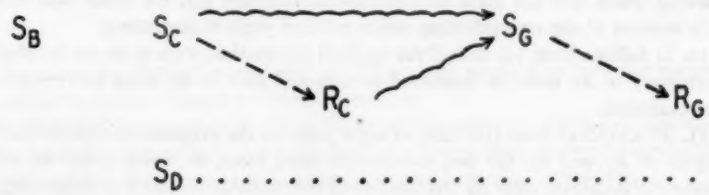
This follows directly from Theorem VI and Definition 9.

VII

Reactions conditioned to a late segment of a stimulus trace will subsequently occur as antedating reactions.

1. Suppose that stimulus S_B precedes stimulus S_C by several times the latency of conditioned reactions; that S_C evokes reaction R_C; that the stimulus trace of S_B extends as far as R_C; that the physical event responsible for S_C, jointly with reaction

R_C, causes S_G; that S_G evokes R_G; and that S_D begins at S_C and persists throughout the remainder of the process.



2. From (1) and Postulate 1, a segment of the stimulus trace initiated by S_B will coincide with R_C.

3. By (1) and Postulate 3, S_G — — → R_G marks a reinforcing state of affairs and follows the coincidence of R_C with the trace of S_B.

4. By (2), (3), and Postulate 2, reaction R_G will become conditioned to a late coinciding segment of the trace of stimulus S_B, *i.e.*, that portion which coincides temporally with R_C.

5. Now, by Postulate 1, stimulus trace S_B at the point of the onset of the stimulus is substantially the same as at the segment conditioned to R_C, except that it is stronger.

6. From (5) and Postulate 2 it follows that once R_C has been conditioned to a late segment of the trace of stimulus S_B with a supraliminal strength, the reaction will be evoked by any portion of the same trace which is as strong as, or stronger than, the segment conditioned.

7. But since, by (1), the initial portion of the stimulus trace of S_B will occur several times the latency of such a reaction in advance of the original point of the occurrence of R_C, it follows from (5) and (6) that after conditioning, R_C will be evoked in advance of the point of its original occurrence.

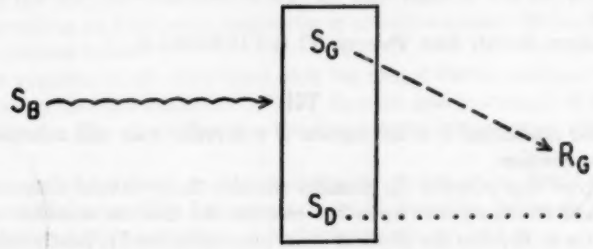
8. From (7) the theorem follows.

Q. E. D.

VIII

Organisms capable of acquiring trace conditioned reactions will be able to execute successful defense reactions.

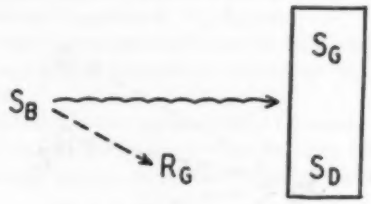
1. Let it be supposed that an organism capable of acquiring trace conditioned reactions is stimulated by S_B, that the external world event responsible for S_B initiates a causal sequence several times the length of a conditioned reaction latency, which sequence terminates in S_G and S_D, the two latter jointly constituting an injury and evoking R_G, a flight reaction, which terminates their impact on the organism; and that the stimulus trace of S_B reaches well beyond the point at which R_G occurs.



2. From (1) and Postulates 1 and 2 it follows that R_G will be conditioned to the trace of S_B.

3. From (1), (2), and Theorem VII it follows that if S_B occurs on a later occasion, reaction R_G will occur in advance of situation $S_G S_D$, which, if it impinges on the organism, will be injurious.

4. But, by (1), R_G is a flight reaction. It follows from (3) that the organism will not be present when the situation otherwise giving rise to $S_G S_D$ occurs and so will escape the injury, thus:



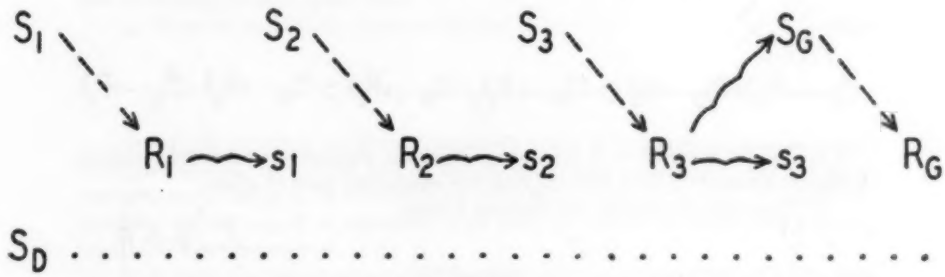
5. From (1) and (4) the theorem follows.

Q. E. D.

IX

In stable behavior sequences terminating in reinforcement, each reaction, in general, becomes conditioned (A) to the proprioceptive stimulus arising from the action immediately preceding it, and (B) to the drive stimulus (S_D), each with an intensity diminishing according to a negatively accelerated rate with distance from the reinforcing state of affairs.

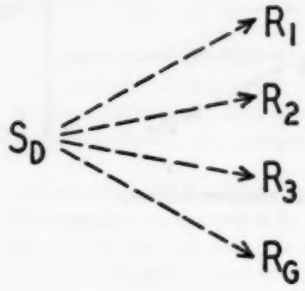
1. Let it be supposed that there impinges on an organism a uniform sequence of external stimuli S_1, S_2, S_3, etc.; that these stimuli evoke in the organism reactions R_1, R_2, R_3, etc.; that these reactions produce (Postulate 6) proprioceptive stimuli s_1, s_2, s_3, etc.; that R_3 by an external causal sequence produces a state of affairs which includes S_G; that S_G evokes R_G; that the combination S_G — — → R_G marks (Postulate 3) a reinforcing state of affairs; and that throughout the sequence there occurs the persisting drive stimulus S_D.



2. By Postulates 1 and 2 the situation supposed in (1) will give rise to an association between each proprioceptive stimulus and the reaction immediately following thus:

$$S_1 \dashrightarrow R_2$$
$$S_2 \dashrightarrow R_3$$
$$S_3 \dashrightarrow R_G$$

3. Also, since by (1) S_D occurs at every point throughout the series, it follows from (1) and Postulate 2 that S_D will be conditioned to every reaction in the series, thus:



4. By (2) and Postulate 2,

$$S_3 \dashrightarrow R_G > S_2 \dashrightarrow R_3 > S_1 \dashrightarrow R_2$$

and

$$(S_3 \dashrightarrow R_G) - (S_2 \dashrightarrow R_3) > (S_2 \dashrightarrow R_3) - (S_1 \dashrightarrow R_2)$$

5. Also by (3) and Postulate 2,

$$S_D \dashrightarrow R_G > S_D \dashrightarrow R_3 > S_D \dashrightarrow R_2 > S_D \dashrightarrow R_1$$

and

$$(S_D \dashrightarrow R_G) - (S_D \dashrightarrow R_3) > (S_D \dashrightarrow R_3) - (S_D \dashrightarrow R_2) > (S_D \dashrightarrow R_2) - (S_D \dashrightarrow R_1)$$

6. But the expressions in (4) and (5) represent negatively accelerated excitatory gradients diminishing with distance from the reinforcing state of affairs.

7. From (2), (3), and (6) the theorem follows.

Q. E. D.

X

A fractional anticipatory goal reaction as a stimulus will tend to bring about the reinforcing state of affairs with which the total goal reaction, of which it is a constituent part, is associated.

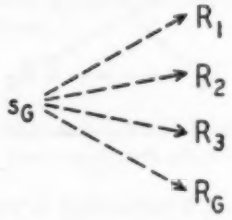
1. Suppose the situation in (1) of Theorem IX with the additional assumption that the goal reaction (R_G) is composed of two components, a major one which cannot take place without the aid of the object represented by S_G and which is incompatible with the several acts of the sequence preceding it, and a minor one (r_G) which is not mechanically dependent on S_G and which may take place simultaneously with the antecedent reactions of the series.

2. Now, by Theorem IX, S_D is conditioned to R_G and, since by (1) r_G is a constituent part of R_G, S_D is also conditioned to r_G.

3. Since, by (1), S_D occurs throughout the series, it follows that it will evoke r_G at all points in the behavior sequence R_1, R_2, R_3, etc.

4. From (3) and Postulates 1 and 6 it follows that the trace of the internal stimulus produced by r_G, *i.e.*, s_G, will tend to occur in conjunction with all the reactions of the sequence R_1, R_2, R_3, etc.

5. Now, each time the situation represented in (4) occurs it is followed (1) by the reinforcing state of affairs marked by S_G — — → R_G, from which it follows by Postulate 2 that s_G will ultimately become associated with all of the reactions of the sequence, thus:



very much as in the case of S_D (Theorem IX).

6. From (5) it follows that s_G will tend, on subsequent occasions, to bring about reactions R_1, R_2, R_3. By (1), R_3 causes S_G, and S_G evokes R_G.

7. But by (1), r_G is a constituent part of R_G which, with its S_G, marks (*i.e.*, is associated with) the reinforcing state of affairs.

8. But if (6 and 7) r_G, through the action of s_G, brings about the inevitable mark of its reinforcing state of affairs (S_G — — → R_G), it must at the same time bring about the reinforcing state of affairs itself.

9. From (7) and (8) the theorem follows.

Q. E. D.

XI

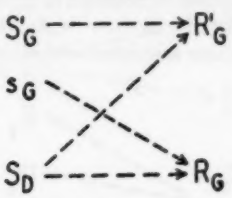
Organisms capable of acquiring functionally potent anticipatory reactions intimately associated with the reinforcing state of affairs, will manifest a weakened tendency to the consummatory reaction if, at the completion of the action sequence, the state of affairs then presented does not permit the occurrence of the complete reaction of which the anticipatory reaction is a constituent part.

1. Suppose that an organism which has been in a situation such as (1) in Theorem IX later finds itself in the same situation with the exception that the terminal conditions, instead of permitting reaction S_G — — → R_G, permit a different reaction, S'_G — — → R'_G, which is appropriate to the same drive (S_D) and is in the repertoire of the organism in question but has a strength only slightly above the reaction threshold.

2. By step (1) of the proof of Theorem IX, together with Theorem IX itself, the customary stimulus complex giving rise to the terminal reaction must be:

3. Now, by (1) and Theorem IX, the s_G of (2) represents r_G, and r_G (Definition 13) is both a reaction anticipatory of, and a fractional component of, R_G.

4. On the other hand, by (1), (2), and Theorem IX, the excitatory tendencies under the changed conditions of the present theorem will be:



5. From (1) and (4) it follows that the excitatory tendencies leading to R_G must be frustrated (Definition 3) which (by Postulate 4) will set up experimental extinction at the point in question.

6. From (5) and Postulate 4 it follows that as a result of the extinction of the tendencies to R_G there will occur simultaneously a weakening of the tendency to reaction R'_G.

7. But by (1) the tendencies to R'_G may be as small as desired and therefore smaller than the generalized extinction of (6), from which it follows that under these circumstances the excitatory tendencies to R'_G will pass below the reaction threshold.

8. From (3) and (7) the theorem follows.

Q. E. D.

Corollary 1.

Organisms will display disappointment.

This follows directly from Theorem XI and Definition 16.

XII

Organisms capable of acquiring anticipatory goal reactions will strive to bring about situations which are reinforcing.

1. Let it be assumed that an organism has acquired a habit-family hierarchy (Definition 17) of two distinct action sequences of the type described in (1) of Theorem X, both originating in the external stimulus situation S_1, terminating in the reinforcing situation S_G — — → R_G and associated with the drive S_D; that the initial acts of one of the sequences are R_I, R_{II}, etc., and those of the other are R_1, R_2, etc.; that the excitatory tendency initiating the sequence beginning with R_1 is dominant over that beginning with R_I, but that the tendency to R_I is far enough above the reaction threshold to survive the weakening effect which would result (Postulate 4) from the frustration of the tendency to R_1.

2. Now suppose that an obstacle is interposed which effectually prevents the completion of R_1 and the remainder of that sequence (1). It follows from Postulate 4 that this excitatory tendency will suffer extinction, with no limit above zero.

3. From (1) and (2) it follows that the sequence beginning with R_I and terminating with R_G will be executed after the frustration of the excitatory tendency leading to R_1.

4. Now, from (1) it follows by reasoning strictly analogous to steps (2), (3), (4), (5), and (6) of the deduction of Theorem X, that s_G will acquire during the acquisition of the habit family the tendency to evoke (*A*) reaction sequence R_1 and all those acts following it in the sequence leading to R_G, and (*B*) reaction sequence R_I together with all those leading from it to R_G.

5. From (2), (3), (4), and Definition 10 it follows that under these circumstances the introduction of a barrier will cause the organism to shift from one behavior sequence *directed* to a reinforcing state of affairs to another *directed to the same* reinforcing state of affairs.

6. But by (5) and Definition 11, when the interposition of an obstacle leads an organism to choose an alternative action sequence *directed* to the same reinforcing state of affairs as that interrupted by the obstacle, the behavior in question is striving.

7. From (6) the theorem follows.

Q. E. D.

Corollary 1.

Organisms will strive for goals.

This follows directly from Theorem XII and Definitions 11 and 12.

XIII

When an organism has attained a reinforcing state of affairs in a situation which, objectively considered, is totally novel, but by means of a member of a previously established habit-family hierarchy, there may follow without specific practice a tendency to a transfer to the new situation of the behavior tendencies represented by one or another of the remaining members of the habit-family hierarchy in question.

1. Let it be assumed that an organism has acquired a habit-family hierarchy (Definition 17) of two distinct action sequences of the type described in (1) of Theorem X, both originating in the external stimulus situation S_1, terminating in the reinforcing situation S_G — — → R_G and associated with the drive stimulus S_D; that the initial act of one of the sequences is R_I and that of the other is R_1.

2. From (1) it follows by reasoning strictly analogous to steps (2), (3), (4), (5), and (6) of the deduction of Theorem X, that s_G will acquire during the acquisition of the habit family the tendency to evoke (*A*) reaction sequence R_1 and all those acts following it in the sequence leading to R_G, and (*B*) reaction sequence R_I together with all those leading from it to R_G.

3. Now, suppose that this same organism in a novel external situation S'_1 and acting under the same drive stimulus S_D reaches, a few times, by the process of trial and error the reinforcing state of affairs marked by S_G — — → R_G, by an action sequence the same as that beginning with R_1 of one of the members of the habit-family hierarchy of (1).

4. From (3) it follows by reasoning similar to steps (2) and (3) of the deduction leading to Theorem X, that r_G will be present throughout the behavior sequence beginning with S'_1.

5. It follows from (4) that there will be a coincidence of r_G and the stimulus trace of S'_1.

6. Since by (3) the coincidence of the stimulus trace of S'_1 and r_G (5) is followed by S_G — — → R_G, it follows by Postulates 1 and 2 that there will be set up the excitatory tendency S'_1 — — → r_G.

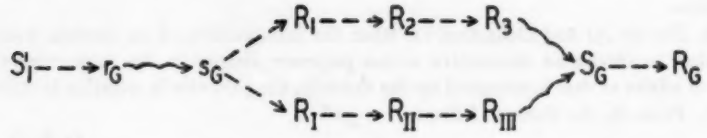
7. From (6), (2), and Postulate 6 it follows that S'_1 will tend to initiate the behavior sequence (omitting internal stimuli after R_1):

$$S'_1 - - \rightarrow r_G \rightsquigarrow s_G - - \rightarrow R_1 - - \rightarrow R_2 - - \rightarrow R_3 \rightsquigarrow S_G - - \rightarrow R_G$$

and also

$$S'_1 - - \rightarrow r_G \rightsquigarrow s_G - - \rightarrow R_I - - \rightarrow R_{II} - - \rightarrow R_{III} \rightsquigarrow S_G - - \rightarrow R_G$$

or, combining the two sequences,



8. Now, suppose that at this point an obstacle is interposed such that R_1 cannot take place, and that s_G — — → R_I is far enough above the reaction threshold to resist the weakening effect of the frustration of the excitatory tendency to R_1. With the competition of R_1 thus removed from the excitatory tendency to R_I, s_G will initiate R_I (7); this will lead to R_{II}, this to R_{III}, etc., and finally to S_G — — → R_G.

9. But the shift from the sequence beginning with R_1 to that beginning with R_I as in (8) is a transfer without specific practice from an old to a new situation because R_I, R_{II}, etc., have never taken place in the external stimulus situation beginning with S'_1.

10. From (9) the theorem follows.

Q. E. D.

The Nature of Adaptive Behavior as Indicated by the Postulates of the Present System

We come now to the second step in our exposition of the procedure which should have been carried out by Eddington and Weiss before they presumed to state the ultimate nature of the more complex forms of adaptive and moral behavior. In this step we turn, mainly for purposes of illustration, to the direct examination of the postulates which gave rise to the system, to see whether they are, in fact, physical or psychic. Let us pass them in review. Postulate 1 states that the physiological effects of a stimulus persist for a certain time even after the stimulus has ceased. Postulate 2 indicates the conditions under which stimuli and reactions become

associated or conditioned. Postulate 3 gives the marks of reinforcing situations. Postulate 4 states the conditions under which associations are unlearned. Postulate 5 gives the conditions under which positive and negative learning are lost. Postulate 6 states the well-known fact of internal stimulation.

At first glance most persons would probably say that these postulates represent the behavior of what has always been regarded as physical. Moreover, the postulates appear to be phenomena of physical structures which most theoretical physicists believe will ultimately be derived, *i.e.*, deduced, by them from electrons, protons, deutrons, etc. According to this view the theoretical physicists will ultimately deduce as theorems from electrons, protons, etc., the six postulates which we have employed as the basis for the deduction of adaptive behavior. If this deduction were accomplished we should have an unbroken logical chain extending from the primitive electron all the way up to complex purposive behavior. Further developments may conceivably extend the system to include the highest rational and moral behavior. Such is the natural goal of science. This is the picture which a complete scientific monism would present. Unfortunately, theoretical physics is very far from this achievement, and judgment regarding its ultimate accomplishment must be indefinitely suspended. At most such a view, attractive as it is, can be regarded only as a working hypothesis.[18]

[18] There is conceivable, however, a kind of experimental shortcut to the determination of the ultimate nature of adaptive behavior. Suppose it were possible to construct from inorganic materials, such as the theoretical physicists have already succeeded in deriving from electrons and protons, a mechanism which would display exactly the principles of behavior presented in the six postulates just examined. On the assumption that the logic of the above deductions is sound, it follows inevitably that such a 'psychic' machine, if subjected to appropriate environmental influences, must manifest the complex adaptive phenomena presented by the theorems. And if, upon trial, this *a priori* expectation should be verified by the machine's behavior, it would be possible to say with assurance and a clear conscience that such adaptive behavior may be 'reached' by purely physical means. A beginning in the direction of such constructions has already been made. See R. G. Krueger and C. L. Hull, An electro-chemical parallel to the conditioned reflex, *J. Gen. Psychol.*, 1931, 5, 262–269; G. K. Bennett and L. B. Ward, Synthesis of conditioned reflex, *Amer. J. Psychol.*, 1933, 45, 339; D. G. Ellson, A mechanical synthesis of trial-and-error learning, *J. Gen. Psychol.*, 1935, 13, 212–218.

But What of Consciousness?

But what of consciousness, of awareness, of experience—those phenomena of which the philosophers and theologians have made so much and upon the priority of which they are so insistent? An inspection of the postulates of the miniature system of adaptive behavior presented above certainly shows no trace of any such phenomena. It is clear, therefore, that so far as that considerable array of complex behavior is concerned, consciousness or experience has no logical priority. In the field of scientific theory no other form of priority is of primary significance.

What, then, shall we say about consciousness? Is its existence denied? By no means. But to recognize the existence of a phenomenon is not the same thing as insisting upon its basic, *i.e.*, logical, priority. Instead of furnishing a means for the solution of problems, consciousness appears to be itself a problem needing solution. In the miniature theoretical system, no mention of consciousness or experience was made for the simple reason that no theorem has been found as yet whose deduction would be facilitated in any way by including such a postulate. Moreover, we have been quite unable to find any other scientific system of behavior which either has found consciousness a necessary pre-supposition or, having assumed it, has been able to deduce from it a system of adaptive behavior or moral action.[19] There is, however, no reason at all for not using consciousness or experience as a postulate in a scientific theoretical system if it clearly satisfies the deductive criteria already laid down.

[19] It is rather hoped and expected that this statement will be challenged. In the interest of the clarification of an important problem, it is desirable that the challenge be accompanied by a formal exhibition of the structure of the system supposed to manifest the critical characteristics. As illustrated above, a theoretical system is a considerable sequence of interlocking theorems, all derived from the same set of postulates. Too often what pass as systems in psychology are merely informal points of view containing occasional propositions which, even if logically derived, would be nothing more than isolated theorems. Some authors are prone to the illusion that such propositions could be deduced with rigor in a few moments if they cared to take the trouble. Others assert that the logic has all been worked by them 'in their heads,' but that they did not bother to write it out; the reader is expected to accept this on faith. Fortunately, in science it is not customary to base conclusions on faith.

If such a system should be worked out in a clear and unambiguous manner the incorporation of consciousness into the body of behavior theory should be automatic and immediate. The task of those who would have consciousness a central factor in adaptive behavior and in moral action is accordingly quite clear. They should apply themselves to the long and grinding labor of the logical derivation of a truly scientific system. Until such a system has been attained on a considerable scale, the advancement of science will be favored by their limiting their claims to statements of their hopes and wishes as such. Meanwhile, one cannot help recalling that for several centuries practically all psychological and philosophical theorists have set out precisely with the assumption of the priority of consciousness or experience. Considering the practically complete failure of all this effort to yield even a small scientific system of adaptive or moral behavior in which consciousness finds a position of logical priority as a postulate, one may, perhaps, be pardoned for entertaining a certain amount of pessimism regarding such an eventuality.

In view of the general lack of the kind of evidence which would be necessary to show the logical priority of consciousness, it may naturally be asked why there is such insistence upon its central significance. While there are many contributing factors, it can scarcely be doubted that an important element in the situation is found in the perseverative influences of medieval theology. During the Middle Ages, and for centuries thereafter, social or moral control was supposed to be effected largely through promises of rewards or punishments after death. Therefore something had to survive death to reap these rewards. Consciousness as a non-physical entity was considered incorruptible and thus immune to the disintegration of the flesh. Consequently it offered a logical possibility of something surviving physical death upon which scores might be evened among the shadows beyond the river Styx. But to be convincing, it was necessary for the thing rewarded or punished to be an essentially causal element in the determination of moral conduct or behavior. Thus it was imperative not only that consciousness be non-physical,

but also that it be the basic factor in determining action. Such a view is incompatible with the belief that the more complex forms of human behavior could be derived without any reference whatever to consciousness. Tradition is strong, especially when fostered by powerful institutions. Accordingly, the frequent insistence on the logical priority of consciousness is not surprising, even when coming from persons who have no clear notion as to the origin of their feelings in the matter.

Thus it can hardly be doubted that psychology in its basic principles is to a considerable degree in the thrall of the Middle Ages, and that, in particular, our prevailing systematic outlook in the matter of consciousness is largely medieval. The situation depicted in a remarkable panel of the fresco by Orozco in the Dartmouth Library gives a powerful artistic representation of this. There, lifeless skeletons in academic garb assist solemnly at the gruesome travail of a reclining skeleton in the act of reproducing itself. What a picture of academic sterility! Fortunately the means of our salvation is clear and obvious. As ever, it lies in the application of scientific procedures. The methodology is old and tried; it goes back even to the time of Galileo. The present paper is, in reality, an exposition of the specific application of this technique in a systematic manner to the problems of complex adaptive behavior. Galileo practiced this methodology at the imminent risk of imprisonment, torture, and death. For us to apply the methodology, it is necessary only to throw off the shackles of a lifeless tradition.

MATERIALISM AND THE CRITERION OF THE PSYCHIC

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The most tenderly cherished conviction of a philosopher appears to be that whatever doubts and perplexities he may find in his universe, he can be certain of his own sensations and ideas, immediately and infallibly certain of everything pertaining to his own mental life. In the words of the late Prof. Royce, "The 'mental elements' exist when they are consciously observed, not otherwise. . . a state of consciousness exists when somebody is conscious of that state. When nobody is conscious of that state, it does not exist." [1] One's own mental life is the one certainty, upon which all knowledge must be based.

This view was expounded at some length by Locke. A man, Locke declared, "cannot think at any time, waking, or sleeping, without being sensible of it. Our being sensible of it is not necessary to any thing but to our thoughts; and to them it is, and to them it will always be, necessary, till we can think without being conscious of it." [2] It is "impossible for any one to perceive, without perceiving that he does perceive. . . . Thus it is always as to our present sensations and perceptions: and by this every one is to himself that which he calls 'self'; . . . For since consciousness always accompanies thinking, and it is that [consciousness] that makes every one to be what he calls 'self,' . . . ; in this alone consists personal identity, *i.e.*, the sameness of a rational being." [3]

It is not an exaggeration to say that this view has been, from the time of Berkeley on, the implicit assumption underlying practically all philosophical speculation. And perhaps

[1] J. Royce, Outlines of psychology, New York: Macmillan, 1906, pp. xvii, 108.

[2] J. Locke, An essay concerning human understanding, 1689, Book II, Chap. I, § 10.

[3] *Ibid.*, Book II, Chap. XXVII, § 9.

few philosophical doctrines have been so ill-founded, or so fraught with mischievous consequences. The fundamental weakness of the doctrine has been pointed out by Melchior Palágyi, who himself was first a mathematician, then a philosopher, and toward the close of his life a psychologist. "Seldom indeed," said Palágyi, "has a theory wrought such havoc among thinkers as Locke's doctrine of the 'internal sense,' by means of which we are supposed to take cognizance of our inner mental operations, just as, by means of our external senses, we gain knowledge of events in the outer world. One sees at a glance that Locke has merely resorted to a sort of poetic simile to help himself out on the question as to how we come to know anything about our own mental activity. As there is an outer eye for seeing outer objects, so there must be an inner eye for perceiving the doings of the mind." [4]

The 'inner eye' or 'inner sense' is surely a metaphor, and a very misleading metaphor. But it was not invented by Locke. It pervaded philosophy and psychology before his time; and it has haunted them ever since. The actual concrete situation which first aroused any psychological-epistemological curiosity was, and still is, the situation in which a living creature *knows* to some extent its environment. The problem was and is: What processes are taking place in the living creature, which constitute this 'knowing' of the environment? This is the original and the only actual question. The 'knower' in our problem is the organism and the 'known' are the objects of that organism's environment.

Starting from this point philosophers and psychologists have said that the objects known are transported into, or reproduced in, the organism, as sensations and ideas; and that a second but invisible knower located inside, called the 'internal sense,' the introspective consciousness, the ego or the soul, there knows them. The original problem is answered by proposing a miniature caricature of it, an analogy, and locating this caricature in the inaccessible interior of the knower, the

[4] M. Palágyi, Naturphilosophische Vorlesungen über die Grundprobleme des Bewusstseins und des Lebens, 2nd ed., Leipzig: Barth, 1924, p. 292.

living organism; where the process of knowing could not be studied, even if the entities now alleged to be known and the entity alleged to do the knowing had not been turned, by a trick of analogical thinking, into the merest verbal fictions. Thus at the dawn of history the visible, tactile and other 'species,' little samples, floated through the sensory pores into the interior of the organism and there encountered something or other that knew how to 'know' them. By the time of Locke and Berkeley it was realized that a flower, to be seen, did not have to pass bodily through the eye and wiggle up the optic nerve: just a little had been learned about sense-organs and their stimulation. But the fictitious sensations, images and ideas of Descartes, Locke, Berkeley and all others of their profession were nevertheless miniature copies of the real things, and they were 'known' by a fictitious entity, the 'soul,' knowing them. And new nonsense had crept in: some of the sensations, the so-called 'secondary qualities,' were not even copies. All of this was taken on faith, and handed on to ourselves, by the nineteenth-century School of Associationists. And again today, the latest wrinkle in psychology, the Gestalt School, clings to the fundamental blunder: perceived objects and events in the outer world are faithfully reproduced, with their spatial and temporal relations preserved, as dynamic centres and lines of force, etc., in the 'brain field,' which is to say the skull. Once there, they are known, of course, by being known. Truly, Berkeley was for once right, when he wrote: "Ancient and rooted prejudices do often pass into principles: and those propositions which once obtain the force and credit of a *principle*, are not only themselves, but likewise whatever is deducible from them, thought privileged from all examination." [5]

To pretend that an organism knows its environment by means of a 'soul' inside the organism knowing sensations and ideas which are also inside the organism, is, in the first place, to explain the process of knowing as being the process of knowing. This tautology does not attack the problem. In

[5] G. Berkeley, A treatise concerning the principles of human knowledge, 1710, Sect. CXXIV.

the second place, it is to transfer the problem of knowledge, which is both psychological and epistemological, from the field of reality where it can be studied, to the realm of fictions where it cannot be studied; where neither the items known nor the soul that knows them nor yet the means by which the soul knows them, can be subjected to scrutiny or criticism. It is to open wide the doors to all conceivable chimæras. This is most desirable: the fictitious soul can be alleged to go to a fictitious heaven or hell. But this childish analogy must be abandoned and forgotten if we propose to address ourselves seriously to the problem of cognition.

The doctrine of emergence states that when entities are combined into wholes, novel properties emerge. The new wholes, so far as we can discern, do not resemble, sometimes not in any respect resemble, their component parts. If this is true, it follows, inversely, that when wholes are analyzed, their component parts will also 'emerge' as novel, and not resembling the whole which they had combined to make. This is of course generally true. Sulphur, oxygen and hydrogen if kept apart do not seem like sulphuric acid; the lines that make a hexagon are not themselves hexagonal; the chemistry of plant tissues seems absurdly irrelevant to one who wanders in a garden. This is always true, I believe, except in the case of wholes of which the constituent parts are not organized but are merely added one to another; the wholes that are merely collections or sums.[6] We may expect, then, that if we analyze the knower and the process of knowing, we shall find that the constituent parts seem quite irrelevant to any such process as knowing; as would be the case if these constituent parts are found to be physiological tissues, such as sense-organs, nerves, and muscles. Such apparent irrelevancy we ought to look for, or even to expect.

Locke declared that our 'internal sense,' by 'being conscious of, and observing in ourselves . . . the operations of our own minds within us,' acquires such ideas as those of 'perception, thinking, doubting, believing, reasoning, know-

[6] This fact has some bearing on v. Ehrenfels' problem of 'form-qualities of higher order.'

ing, willing, and all the different actings of our own minds.' [7] This 'internal sense' he calls also, in various places, mind, understanding, consciousness, Self, and soul; but most commonly he refers to it by means of the pronoun I. Although this 'I' can perceive and reflect upon all of its own actions, I have not found that Locke anywhere states that the 'I' can actually perceive or become conscious of itself. On the other hand Locke makes a most extraordinary admission: "We have the ideas of *matter* and *thinking*, but possibly shall never be able to know whether any mere material being thinks or no. . . . For I see no contradiction in it, that the first Eternal thinking Being, or Omnipotent Spirit, should, if he pleased, give to certain systems of created senseless matter, put together as he thinks fit, some degrees of sense, perception, and thought." [8] It was for this, among other things, that Bishop Stillingfleet [9] disputed with Locke so hotly. Locke left it an open question whether matter, when suitably put together, can become a conscious mind, an internal sense, an understanding, a soul, and an Ego or I.

Hume [10] and Priestley [11] both declared that they could not by introspection find any trace of the internal knower, the soul or ego. P. Souriau, in the best article on just this point with which I am acquainted, asserts that there is no immediate knowledge of the self, and that any such immediate 'knowledge' as Locke assumed, of our own mental processes, is quite at variance with the facts.[12] Ernst Mach and Lichtenberg agree that we ought to say "It thinks," etc., and not "I think"; just as we say, "It rains." "Let there be no mention," says Mach, "of the so-called unity of consciousness." [13] Mauthner proposes that we accustom ourselves to say, "it

[7] *Op. cit.*, Book II, Chap. I, § 4.

[8] *Ibid.*, Book IV, Chap. III, § 6.

[9] E. Stillingfleet, Bishop of Worcester, A discourse in vindication of the doctrine of the trinity, London: Mortlock, 1697, pp. 239–241.

[10] D. Hume, A treatise of human nature, 1739–1740, Book I, Part III, § 5; Part IV, §§ 5, 6.

[11] J. Priestley, Disquisitions relating to matter and spirit, 2nd ed., 1782, vol. I, Sect. XI, pp. 132–135.

[12] P. Souriau, La conscience de soi, *Rev. Phil.*, 1886, 22, pp. 449–472.

[13] E. Mach, The analysis of the sensations, *Monist*, 1890, 1, pp. 64–65.

thinks in me"; since the familiar pronoun 'I' is merely an empty word.[14] I hold with the six authors just cited that the facts of psychology require us to give up the traditional notion of the self and the traditional notion of an immediate introspective knowledge of the self.

As revealing the merely dialectical rather than empirical quality of this doctrine, it has been observed that if the very essence and existence of all mental processes and contents (including sensations and ideas) is dependent on their 'being perceived,' then of course this 'being perceived' must be in turn perceived, and this again perceived, and so on forever. "Taking Locke at his word, then, . . . ," as T. H. Green has said, "the beginning of intelligence consists in having an idea of an idea of an idea of sensation." [15] And, dialectically, that would be but the barest beginning. Remarkably enough, Spinoza did not flinch from this fantastic implication, frankly asserting that " . . . if a man knows anything, he, by that very fact, knows that he knows it, and at the same time knows that he knows that he knows it, and so on to infinity." [16]

If now we can forget the mythical inner self, the soul in the pineal gland which forever mutters to itself "I know that I know that I know that I know," and if we can open our eyes to obvious facts we shall see that the only knower, the only active self that there is, is the physical body simply; that and nothing else. As Alexander Bain wrote, some eighty years ago: "The organ of mind is not the brain by itself; it is the brain, nerves, muscles, and organs of sense. . . . It is, . . , an entire misconception to talk of a *sensorium* within the brain, a *sanctum sanctorum*, or inner chamber, where impressions are poured in and stored up to be reproduced in a future day. There is no such chamber, no such mode of reception. . . . We must thus discard forever the notion of the *sensorium commune*, the cerebral closet, as a central seat of mind, or receptacle of sensation and imagery. . . . Our present insight

[14] F. Mauthner, Beiträge zu einer Kritik der Sprache, 3rd ed., Leipzig: Meiner, 1923, vol. I, pp. 42, 67, 258, 638.

[15] T. H. Green, General introduction, to Hume's 'A Treatise of Human Nature, London: Longmans, 1890, vol. I, p. 9.

[16] B. de Spinoza, Ethics, transl. by R. H. M. Elwes, Part II, Prop. XXI, note.

enables us to say with great [p. 62] probability, no [nerve-] currents, no mind. The transmission of influence along the nerve fibres from place to place, seems the very essence of cerebral action. This transmission, moreover, must not be confined within the limits of the brain: . . . it is doubtful if even thought, reminiscence, or the emotions of the past and absent, could be sustained without the more distant communications between the brain and the rest of the body—the organs of sense and of movement." [17]

William James, in the last work which he was ever to write, reaffirms an opinion which he had expressed in his Principles some thirty years previously: "The world experienced (otherwise called the 'field of consciousness') comes at all times with our body as its center, center of vision, center of action, center of interest. Where the body is is 'here'; when the body acts is 'now'; what the body touches is 'this'; all other things are 'there' and 'then' and 'that.' . . . The body is the storm center, the origin of coördinates, the constant place of stress in all that experience-train. Everything circles round it, and is felt from its point of view. The word 'I,' then, is primarily a noun of position, just like 'this' and 'here.' [18]

So far I shall appear to have been developing, from a certain angle, merely the thesis of Behaviorism. And so far I do assent to that thesis. But so far and no farther.

From our present position we must reject that statement which I quoted at the outset from the late Prof. Royce, namely, that "The 'mental elements' exist when they are consciously observed, not otherwise . . . a state of consciousness exists when somebody is conscious of that state." Prof. Royce was assuming the internal sense, or soul. But I at least, in contrast to the behaviourists, *would* assent to Bertrand Russell's dictum, that "The occurrence of the content of a thought constitutes the occurrence of the thought." [19] To quote again from Souriau's article of 1886: "Suppose a

[17] A. Bain, The senses and the intellect, London: Parker, 1855, pp. 61–62.

[18] W. James, The experience of activity, in 'A Pluralistic Universe,' London: Longmans, 1909, p. 380, note.

[19] B. Russell, The analysis of mind, London: Allen and Unwin, 1921, p. 17.

luminous object placed in front of me, my eyes open and directed toward it: I see it. Have I any consciousness of seeing it? No. Vision is an absolutely unconscious act. As long as I perceive the object, my attention is wholly directed to it, and it is impossible for me to shift my attention in the least to the act by which I am perceiving the object. . . . If I want to take note of the way in which I perceived it, I cease to perceive it. . . . It is impossible for us, then, while we are experiencing a sensation, to discover in ourselves an idea, image or intellectual process of any kind which should have that sensation for its object. Sensations are, therefore, by their very nature unconscious." [20] Thus a sensation does not have to be a second time sensed, nor does a perception have to be a second time perceived, nor an idea to be a second time ideated. Thinking and knowing are processes in their own right: to think is not to think that I think that I think; and to know is not to know that I know that I know that I know—into all eternity. In short, these considerations convey the gentle hint that philosophers and psychologists might improve themselves by learning not to stammer.

Now that the ground is somewhat cleared of rubbish, we may be able to see that we are facing two genuine problems. The first is, What is that process or act which creates perceptions, ideas, and other mental contents? The second problem is, What is that additional process or act which brings it about that a part of these mental contents are said to be in introspective consciousness, or by ellipsis, in consciousness? For it is true that there is a process, called 'conscious introspection,' which Locke and his followers mistook for the more general process of cognition, and which is unquestionably responsible for the ancient delusion about an 'internal sense.'

As Souriau has said, "Mental images commonly appear and disappear quite of themselves, without any intervention on our part. . . . The force which creates our ideas and brings them into consciousness, remains itself unconscious." [21] Since, as we have seen, *mind* must be carefully distinguished

[20] P. Souriau, *op. cit.*, pp. 455–457, 472.

[21] *Ibid.*, pp. 457, 450.

from *consciousness*, Souriau would better have said, "the force which creates all of our mental contents, is itself never a mental content." And in the same vein Russell has remarked, in a passage from which I previously quoted, "According to him [Meinong] there are three elements involved in the thought of an object. These three he calls the act, the content and the object. . . . The first criticism I have to make is that the *act* seems unnecessary and fictitious. The occurrence of the content of a thought constitutes the occurrence of the thought. Empirically, I cannot discover anything corresponding to the supposed act; and theoretically I cannot see that it is indispensable." [22] Quite so: there is no mental 'act' and there is no conscious 'act' (though we loosely speak as if there were); and Russell's 'empirical' observations are never extended to include the thinker's body, his physiological mechanism. The mental contents may flow, they do not 'act.' The physical body is the actor, and it is the neuro-muscular mechanism of that body which creates the mental contents and governs in every respect what they shall be and in what succession they shall flow; as was indicated in the passages which I have quoted from Alexander Bain and from William James. There is no 'act' where there is not physical mass, and the contents of the mind are imponderable. Thus we are led to the view, as Georg Wernick has remarked, "that mental presentations are most closely related to acts of the will, that the two are in fact at bottom identical." [23] In short, mental contents (both ideas and volitions) are products of or, if you prefer, aspects of neuro-muscular activities. It is here that we must find our criterion of the psychic.

To turn now to our second problem, that of the distinction between mental contents and introspectively conscious mental contents, we shall find it an advantage that we have renounced introspectibility, or immediacy to an 'internal sense,' as our criterion of the psychic. For the testimony of actual experience is emphatic and unanimous: every school of psychology

[22] B. Russell, *op. cit.*, pp. 16–17.

[23] G. Wernick, Empfindung, Wahrnehmung und Vorstellung, *Zsch. f. Phil. u. philosophische Kritik*, 1911, 141, pp. 171–172.

since certainly before the time of Herbart has found that by far the greater portion of the sensations, ideas and processes which we must call 'mental' never become explicitly conscious; they are not perceived, and cannot by any known process of introspection be perceived. So that for generations the misguided psychologist, imagining his field of study to be the field of introspective consciousness, has found himself studying indubitably mental phenomena which he was forced to call non-introspectible and non-conscious. The empirical testimony on this point is overwhelming. It is well summed up by Freud, when he says, "that mental processes in and of themselves are unconscious, and the conscious are merely isolated acts and passages in the total life of the mind." [24] But so long as the contents of the mind are defined in no other way than as that which the inner eye of introspective consciousness perceives, then their very *esse* is *percipi*, their only being is to be perceived. And psychology finds itself in the stultifying predicament of being the science of unconscious consciousness. And that is exactly where this vicious old theory would leave us.

The distinction which I believe to be the true one between mental contents and introspectively conscious mental contents has been formally stated and adopted, so far as I am aware, by only one author—Melchior Palágyi. The distinction which he draws [25] is that any mental content becomes a so-called 'conscious' mental content if, and only if, the physiological process which creates the mental content also starts into action the speech-mechanism; in other words, it is the 'verbal report' pronounced upon a preceding mental state which brings that state 'to consciousness.' This is a revolutionizing doctrine. Distant rumblings of it can be detected in Samuel Butler, who more than once uttered such opinions as the following: "Knowledge is in an inchoate state as long as it is capable of logical treatment; it must be transmuted into that sense or instinct which rises altogether above

[24] S. Freud, Vorlesungen zur Einführung in die Psychoanalyse, Leipzig: Heller, 1918, p. 9. See also pp. 10, 104, 159, 315, 335–339.

[25] M. Palágyi, *op. cit.*, Vorlesung XX.

the sphere in which words can have being at all, otherwise it is not yet vital." [26] Bertrand Russell went, on at least one occasion, much further. "I believe that the discovery of our own motives can only be made by the same process by which we discover other people's, namely, the process of observing our actions and inferring the desire which could prompt them. A desire is 'conscious' when we have told ourselves that we have it. A hungry man may say to himself: 'Oh, I do want my lunch.' Then his desire is 'conscious.' But it only differs from an 'unconscious' desire by the presence of appropriate words, which is by no means a fundamental difference." [27] Precisely these words might have been written by Palágyi; but Russell did not realize their full import, as is shown by his subsequent arguments.[28]

To accept these two solutions would be to revise fundamentally both our philosophy and our psychology. Many of us, as probably Russell, do not believe in an 'internal sense'; yet we are not prepared to accept the lesson from 'Emergence,' that a genuine analysis of the mind and the cognitive process will inevitably analyze these into things and processes which are neither mental nor cognitive. Any pretended analysis that does not do this is not an analysis but merely, once more, an analogy. The theory of the 'internal sense' with its variants in the forms of Ego, Self, Soul, etc. penetrated the very atmosphere of psychology and philosophy centuries ago; it is implicit at every turn in psychological and philosophical terminology. Thus, for instance, Prof. Müller-Freienfels has a series of articles on imageless and wordless thought, which do more than any other articles that I know of to disprove the existence of any internal sense or subjective field of consciousness. One of his main concepts is 'Einstellung,' used in such a way that it could be translated only as 'motor-set.' Several times he makes a categorical statement such as this: "all actual mental processes are of a motor character. And this motor is not merely an accompanying phenomenon,

[26] S. Butler, Life and habit, London: Fifield, 1877, p. 29. *Cf.* Chap. II entire.
[27] B. Russell, *op. cit.*, p. 31.
[28] *Cf. ibid.*, p. 289.

but it is the essence [das Wesen] of all mental activity." [29] Yet he feels it necessary to disclaim any intention of heresy, as follows: "Of course I need hardly point out that I here identify movements and consciousness not in the sense of materialism, but rather is it in the sense of parallelism that I see in one the physiological correlate of the other." [30] It is very hard to revise radically our ingrained habits; very hard to achieve a radically new orientation. Can any of us bear to give up 'sensations' and substitute therefore mere qualities? Can we quite rid ourselves of the idea that proprioceptive afferent impulses are 'kinæsthetic sensations' being conducted to the sensorium for inspection and appraisal? Can we fully realize that 'voluntary' motor impulses do not originate in some exceptionally significant, some 'pontifical,' region of the cerebral cortex? I believe that an honest self-examination will reveal to most of us that the answer is No. The theory of the 'internal sense' with its ramifications and variants occupies among us today the same position which was held for a hundred years among chemists by the 'phlogiston' theory, and which is held today among physicists by the theory of 'luminiferous æther.' Everyone knows that this theory is impossible and untrue, and yet everyone continues to think and to argue as if it were possible, and possibly true. The fact is, as Émile Meyerson [31] has shown, that no theory is ever given up merely because it is well known to be ridiculous; it *may* be given up when some new theory has been proposed to replace it. Even scientists, it seems, require some theory through which to view facts.

Nevertheless, there has been a drift in the direction which we have been considering; that is, toward a definitely motor theory of mental processes, and, among the students of imageless thought, toward the abandonment of introspection as a criterion of mental activity. Most of these students, as for instance Theodor Lipps, have had little or no knowledge

[29] R. Müller-Freienfels, Beiträge zum Problem des wortlosen Denkens, *Arch. f. d. ges. Psychol.*, 1912, 23, p. 334.

[30] R. Müller-Freienfels, Vorstellen und Denken, *Zsch. f. Psychol.*, 1912, 60, p. 430.

[31] É. Meyerson, De l'explication dans les sciences, Paris: Payot, 1927, p. 91.

of physiology, and certainly commenced their studies with no bias toward physiology. Everyone recognized at once that Lipps's *empathy* (Einfühlung), though not so intended by Lipps, was the description of a neuro-muscular process. The *determining tendency* (determinierende Tendenz) of N. Ach and the *task* or *problem* (Aufgabe) of H. J. Watt seem to me unmistakably motor, and ultimately neuro-muscular, terms. The term *set* or *adjustment* (Einstellung) as used by Betz and Müller-Freienfels is consciously intended by them to mean what the physiologist Exner would have called '*Zustand der Bahnung*,' or (as I should say) motor-set. Similarly the *imagined movement* (eingebildete Bewegung) of Palágyi is intended by him to mean motor-set or play of tonus, in contrast to his other fundamental concept, *executed movement* (ausgeführte Bewegung).[32]

Of all the studies of this type with which I am at all acquainted, those of Palágyi and Müller-Freienfels seem to me the most instructive. These two are in practically complete agreement; and I should like to mention a few of their conclusions, with some explanatory interpretation of my own. The underlying thesis of both is stated in the words of Müller-Freienfels which were quoted above: "all mental processes are of a motor character." An executed movement in response to an object produces awareness of the object; a mere motor-set, involving a slightly heightened tonus of the same nerves and muscles, does not produce awareness. But the motor-set is the very mechanism of imageless and in general of non-introspectible thought; for these motor-sets, by producing summations and mutual inhibitions, determine the direction of thought and lead eventually to executed movements.

There is under some conditions an experience of awareness which leaves scarcely any, even evanescent, memory-trace, and which does not stimulate the word-articulating, or speech,

[32] The following three papers are of special interest: G. Dubreuque, L'intuition motrice, *Rev. Phil.*, 1898, **46**, pp. 253–292; E. Posch, Umriss einer realistischen Psychologie, *Arch. f. d. ges. Psychol.*, 1923, **44**, pp. 191–243; S. Alexander, Art and the material, Manchester: University Press, 1925, 32 pp.

mechanism. By the very nature of this experience, it is hard to make sure that it has taken place, and hard to say anything about it. Whereas all so-called 'introspective consciousness' involves both immediate retrospection (memory) and verbal report. By its very nature the experience in question is doomed to be called 'unconscious,' if not indeed to be forever ignored by psychology. It can not be attested by the experiencer himself.

Perhaps the best way to realize what is here meant, is to recall an occasion when one was suddenly aroused from sleep, and barely remembered the end of an interrupted dream, which faded so instantly that the only possible verbal report was a bare 'Something.' Yes, and something mental. Suppose, now, *that one had not been awakened!* Would that have made it that the dream never took place? Certainly not, the dream as vivid as you please, would have *gone on* taking place.

But this dream was not 'given to a conscious self,' as the 'internal sense' theory would have it. If the dreamer, on waking, says "I was having a dream," he has unpardonably falsified his report. The dream-content included no 'I' that was having the dream, and no such conceptual predicate as 'dream.' Any the barest verbal report is false to the content of the dream; it is, namely, a subsequent mental act superimposed on, and displacing, the original dream-content.

There is another mental state which also permits of no retrospective survey and no verbal report: this is the state of extreme and smoothly flowing mental concentration; that which Goethe liked to call 'pure activity' (die reine Tätigkeit).[33] Here, where the mind is working at its highest efficiency, there is no immediate memory, no intercurrent reminiscence, and no sense of an active Self. One emerges with the task completed, and no memory save that one has been busy. If, however, the state of concentration is interrupted by the meeting with some obstacle, some impending doubt, or again by an extraneous interruption, then at once retrospection returns and an awareness of the impeded 'Self.'

[33] *Cf.* A. Schopenhauer, The world as will and idea, Eng. transl., 1886, vol. II, Chap. XIX, p. 434.

Such non-introspectible experiences are mental contents, full and unqualified, for both Müller-Freienfels and Palágyi. Palágyi, in particular, goes on to show [34] that it is primarily the verbal report, formulating in sounds commonly used for social communication and at the same time utterly superseding the actual contents reported on, which most persons *mean by* '*consciousness.*' [35] *Unbewusst* is merely *unbeachtet* (M.-F.). In fact, I think that if one undertakes to introspect on some present sensory experience while at the same time firmly inhibiting all tendencies to verbal articulation, one will feel that the introspection is curiously frustrated; the experience remains just objects and qualities. Or if one makes the same attempt with some remembered imagery, the experience seems to remain image, until some verbal articulation is permitted to start up.

This verbal articulation is not the 'Self perceiving itself,' nor one state of consciousness 'observing' another state of consciousness: it is merely a word-motor response (yielding awareness) which has been set in action by a preceding motor response (which had yielded its own but different awareness). This and some other points may be made clearer by an illustration taken from Müller-Freienfels.[36] As I work, a loud crash occurs in the next room, and I find myself automatically hurrying into that room and looking about inquiringly: not a word, image or other introspectible content in my mind. Yet this act is purposeful, intelligent, and my inquiring glance is not impartial, for only something *amiss* will arrest my eye-movements. If someone sets me to 'introspect,' by asking "Why did you dash in here so hurriedly?," the answer will probably be "I came to see what had happened." It is not hard to see how my previous movements and subconscious motor-sets prepare the speech mechanism for such an utterance. But how remote this is from any such nonsense as that about 'the Self observing itself'! The crash when first heard

[34] M. Palágyi, *op. citat.*, Vorlesung XX.

[35] Interesting in this connection is the essay by E. Rignano, "What is consciousness?" in his Essays in scientific synthesis, Chicago: Open Court, 1918, pp. 133–150.

[36] R. Müller-Freienfels, Beiträge etc., pp. 320–322.

may set up in me the image of a certain blue jar which I well know to be in the next room, perhaps even a phantasm of the blue jar falling from its customary position and breaking to pieces on the floor. If so, my eyes when I reach the other room will look first toward the accustomed position of the jar, or else toward a spot on the floor directly beneath the customary position. My 'introspection' will be, "I thought the blue jar fell and broke."

Of special interest, perhaps, is Müller-Freienfels' doctrine of the *concept* and the *judgement*. A concept is the verbal formulation of the *synthetic* perception. If when walking with a friend I am deep in conversation and turn out to avoid a tree or I step over a mud-puddle without verbal comment, I have 'perceived' the tree and mud-puddle without really forming the concept 'tree' or 'mud-puddle.' The judgement, similarly, is implicit in a perception ; it is the verbal formulation of an *analytic* perception. If I behold a rose-bush and say "the rose is in bloom," the fact of being in bloom is selected out, and other features of the scene, which could also be put into words, are neglected. I might have said, instead, that "this rose is red" or that "this blossom has just fallen off":—two further judgements. The feature of a situation which is selected and then transcribed in words as a 'judgement,' is selected by exploring movements of the eyes; and these movements are determined, as Müller-Freienfels says, not by an ego or soul, but by the excitations which are at the moment playing through the sensori-motor mechanisms.[37] The analyzed components of mental process are not conscious contents presented to a Self; they are temporal and spatial configurations created by muscular movements and motor-sets. Both Palágyi and Müller-Freienfels insist firmly that the mental content is an aspect of the movement; it never precedes nor causes any movement; it accompanies or perhaps follows the movement. The retrospective nature of introspection makes it practically impossible to decide which. But

[37] R. Müller-Freienfels, Die Bedeutung der motorischen Faktoren etc., *Vierteljahrssch. f. wiss. Phil. u. Soziol.*, 1914, **38**, I, pp. 215–253; II, pp. 335–371. Specially pp. 356–359.

in any case all *causation* is strictly in the physiological mechanism.

The Self, so far as it is experienced, is merely the perception of parts of one's own body, or such frivolous trifles as James pointed out—feelings of frowning, peering, etc. The physical body is the Self. Here it is to be remembered that the frequent use of the pronoun 'I' is due to a, perhaps unfortunate, linguistic convention. It would be more accurate to say that "it thinks in me" than that "I think." However, this need not occasion any confusion. When I say that "I digest my food well" or that "I wear number 7 shoes," nobody is deceived into thinking that my *entire* body, and still less that any spiritual inner being, is implied.

For Palágyi and Müller-Freienfels, as also for Freud, "mental processes are in and of themselves unconscious, and such of them as become conscious are merely isolated acts and passages in the total life of the mind." [38] The supplementary process which makes them 'conscious' is merely a series of verbal reports—always very fragmentary, verbal and so not representative, extremely capricious, and often entirely irrelevant. As to the actual, non- 'conscious,' contents of the mind, Müller-Freienfels is rather more explicit than Palágyi. Apart from direct perception the mental contents seldom reproduce reality, not even in a faded or impoverished way. Yet at times, and particularly when active thought finds itself impeded, visual images, more or less representative of actual perceptions, are produced momentarily. It may be that a few persons are capable of auditory imagery. Müller-Freienfels doubts the existence of any other imagery. For the rest, thought consists of active 'transitive' states with frequent 'halting-places' interspersed. Here Müller-Freienfels makes open and generous acknowledgement to William James's studies on 'The Experience of Activity.' [39] One will readily see what is meant by transitive states and halting-places if one will close one's eyes and *imagine* the shape of a triangle or quadrangle, or imagine 3 times 2.

[38] S. Freud, *op. cit.*, p. 9.

[39] W. James, A pluralistic universe, London: Longmans, 1909; Essays in radical empiricism, London: Longmans, 1912.

We have reason to believe that in reality there is no movement without some physical body that moves. But the mental experience of motion with nothing moving is familiar. It can be observed in the grossest way in some phases of the *phi* phenomenon. It is the muscular *motion* precisely that creates awareness; the limb or other bodily member which does the moving is not included in this awareness.

From this radically different point of view the mental patterns and the varieties of mental activity cannot be studied merely by introspection. We are no longer dealing with imponderable parcels of 'consciousness' presented to a mythical 'internal sense.' And 'immediacy' to such an 'inner sense' is no longer our criterion of the psychic. The mind can now be studied only by studying the sensori-motor activities of a living organism in a concrete situation. The final court of appeal as to whether an 'idea' has occurred is now not the irresponsible vapourings of 'systematic introspection,' but the study, as complete as the living tissues and our available apparatus permit, of what muscular contractions and what interplay of neuro-motor tensions have taken place. This is a formidable programme. But it is by no means hopeless.

I am aware that a position so out and out materialistic is excessively repugnant to all philosophers, and to almost everybody else. But let us see. The late M. Émile Meyerson remarked somewhere, that everybody delights to lambaste materialism but nobody stops to say what materialism is. What is the actual distinction between idealism and materialism?

The classical distinction has been, I suppose, that according to one view the 'ultimate constitution' of the universe is idea, and that according to the other it is matter. If these words mean anything, they mean that the 'ultimate' or least discoverable particles of the universe are composed of idea-substance or, in the other case, of material substance. The two adjectives ideal and material cannot refer to the way in which the least particles are organized, at least they cannot mean this for idealists, since obviously, and even according to Berkeley himself, they are *organized* into the mode called

'matter.' Now any predicate (idea, matter, or whatever other) assigned to the ultimate substance of the universe, becomes a universal predicate. By denoting everything it can cannote nothing, and can therefore mean nothing. In fact, this whole train of thought is not dynamic thinking at all; it is gross *substance* thinking: it is, in other words, riding an analogy.

Secondly, all considerations about 'ultimate' constitution or substance are irrelevant to our issue. The knower is a concrete material body in a concrete material environment, and the cognitive relation exists between the two. No question about ultimates has any bearing on the matter; just as for Berkeley such questions had no bearing when he was experimenting to discover the general efficacy of tar-water. Berkeley's pretended escape from the concrete situation in which any and all cognition takes place, and his plunge into subjectivism, was a piece of charlatanry, and was suitably stigmatized ever after by his bishop's mitre. I have dealt with his argument for sensationism elsewhere.[40] I make, then, no apologies for a psychology which is out and out as materialistic as you please; no deductions from it; and no qualifications of it. It proceeds, and in my opinion successfully, to define mind in terms of matter; or, more specifically, in terms of nerves and muscles: terms which mean far *more* than the mere word 'matter.'

The fundamental distinction between materialism on the one hand and all varieties of idealism on the other has to be drawn at exactly the point where subjective idealists departed from the path of truth and lapsed into their subjectivist dream. This distinction was well stated in 1888 by Friedrich Engels: "Those philosophers who asserted the primacy of spirit to nature . . . comprised the camp of idealism. The others, who regarded nature as primary, belong to the various schools of materialism. These two expressions, idealism and materialism, primarily signify nothing more than this;"[41] Some water has flowed under the bridge since 1888, and I

[40] E. B. Holt, The argument for sensationism as drawn from Dr. Berkeley, PSYCHOL. REV., 1934, 41, pp. 509–533.

[41] F. Engels, Ludwig Feuerbach and the outcome of classical German philosophy, New York: International Publishers (no date), p. 31.

should now reword what is nevertheless the same distinction, as follows: Those philosophers who acknowledge the existence of *some extra-mental* reality belong to the materialist tradition; those who do not admit the existence of an extra-mental reality are subjectivists. Since the days of Protagoras and Plato, Democritus and Aristotle, this has been the most significant difference between philosophical schools.

That all subjective idealists far surpass any materialist in learning, sophistication and general mental acumen, is freely conceded by all subjective idealists. As shown by their printed remarks,[42] they conceive materialists to be a species of filthy Yahoos who eagerly lick up crumbs from the none too festive boards of Lamettrie, Holbach and Büchner. Since, however, subjectivists, so far as I can discover, limit their own reading to the Sacred Texts (Berkeley, Kant, Hegel, Bradley, etc.), the Proceedings of the Aristotelian Society, and a few stray tracts on deity, prehension among the monads, and holism, they run the risk of not knowing exactly what the materialists have been doing. For materialism since Vaihinger, since Freud, since Mauthner, and since the late M. Émile Meyerson is not precisely what it once was.

In fact, the strictly materialist school of psychology has discovered that our physiological apparatus of perception and thought is exceedingly untrustworthy as an instrument of cognition. It habitually distorts, mutilates, and disguises that which it should be perceiving. It subtracts from the objective reality, and with the remainder it fuses inseparably a vast amount of unreality of its own motor creating (subjective unreality). And it does this in several characteristic ways. Some of these are called wishful, analogical and teleological thinking, fictions (*als ob*), hypostatizations, and so on; to say nothing of the mere sensory illusions, hallucinations, and others of that order. These characteristic distortions are strikingly analogous to the Kantian 'categories,' that utterly distort both the intuition and the understanding of things-in-themselves.

[42] Compared with the maledictions of Bishop Berkeley, the following words of the late Fritz Mauthner sound like commendation: ". . . since Kant, materialism can no longer be taken seriously . . . one might call materialism the dunce's philosophy." (F. Mauthner, *op. cit.*, vol. III, p. 550.)

Our neuro-muscular apparatus of cognition is held down to perceiving fairly truthfully only where it meets physical objects as obstacles. Collision or *choc* holds it fairly well on the straight and narrow path. So that we know tolerably well the physical objects which immediately surround us. Since our nerves, muscles and other physiological tissues are precisely such objects, we know the mechanism of cognition itself tolerably well. Happily enough, it is perfectly clear to the materialistic psychology of which I have been speaking, *why* this mechanism inevitably interposes most of those categories of delusion which I just enumerated.

'Matter' is that extra-mental reality which we encounter as obstacle. The materialist is not interested in the so-called 'ultimate nature of matter,' nor in any other ultimates or absolutes. Vaihinger, Mauthner, and Émile Meyerson called themselves idealists. Yet Vaihinger everywhere assumes, and occasionally permits himself to use the word, 'reality'; meaning an extra-mental reality. Mauthner does the same. Émile Meyerson calls it 'le divers,' the alien. Kant used the expression 'thing-in-itself.' All three terms are excellent; and mean exactly the same thing. And no modern materialist thinks of matter in any more explicit sense: not even, I believe, Dr. Niels Bohr. It is a pity that Kant did not at all times hold steadfastly to his things-in-themselves. But that is a chapter of history which, in my opinion, Eduard von Hartmann [43] has satisfactorily settled.

In this paper I have tried to point out that the so-called 'immediate awareness by an internal sense' cannot be used by psychology as a criterion of the psychic; that introspection is merely a verbal commentary which supersedes and displaces a state of awareness which it never successfully describes; that awareness is created by motor response; that these motor responses are the only genuine criterion of the psychic; and, lastly, that an ultra-materialistic psychology is able to account for the epistemological categories of delusion.

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[43] E. v. Hartmann, Das Ding an sich und seine Beschaffenheit, 1871. Second and third editions of the same, entitled 'Kritische Grundlegung des transcendentalen Realismus,' 1875 and 1885.

THE CONCEPTS OF LEARNING AND MEMORY

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In view of the emphasis being placed on learning in recent years, it is surprising that there should be almost as many definitions as there are writers on the subject. Some of the general confusion results from differences in systematic psychological positions, and on this matter it is sometimes difficult for one psychologist even to appear reasonable to another. A more important explanation of the multitude of opinions lies in clearer matters of fact which can also be discussed profitably.

In the first place, there has admittedly been an arbitrary selection of subjects used in learning experiments. The assumption has been made by different writers on different occasions that the psychology of learning should be derived from and built up on phenomena observed, for example, in human subjects between 14 and 21, in children between 6 and 14, in children between 2 and 6, in subhuman primates, in subprimate mammals, and so on. The concept of learning, however, is broader than all of the processes observed in any one animal species or in any limited range of human ages. Students of learning will have to take a more generous attitude towards the results obtained by others who have used subjects that are quite different from the ones they employed.

In the second place, there has admittedly been an arbitrary selection of the forms of learning on which to base and from which to derive the general principles of learning. Among these varieties of learning may be mentioned the conditioned response, paired associates, learning to select one of two responses, the acquisition of perceptions, learning to speak, learning to read, redintegration, multiple choice, relatively simple serial learning, learning logical material, the association of ideas, the acquisition of habits, and learning

skilled manual performances. It seems more or less obvious that *all* of these processes and many others are forms, varieties, or manifestations of learning. The conditioned response is *a* form of learning, but so also are acquiring meanings, learning a new language, and learning to play musical instruments. The assumption or claim that the only or principal form of learning—the single form which tells practically the whole story—is the learning observed in maze situations, in multiple choice procedures, in the conditioned response, in 'trial and error,' *or* in insight situations, is a gross error. The concept of learning which we wish to describe is broader than any one of the several existing forms of learning, and we have included in our concept of learning all of those processes which in ordinary discussions are frankly admitted to be learning processes.

An acceptable definition of learning should include those psychological activities which it is desired to include, and exclude those which it is desired to exclude. The definition should describe the process in question in a general way, and distinguish it from all other psychological and non-psychological processes. The concept should aid rather than hinder in ordering and classifying the facts which have already been discovered, and the value of the definition would be greatly increased if it led to worthwhile research in the future. We have tried to fulfill all of these requirements in the following definition.

Learning is the establishment or strengthening of neural connections between stimulating processes and responding processes as a result of accompanying or immediately preceding psychological activities.

Stimulating patterns, functional neural connections, and responding patterns are involved in all kinds of learning, and one should refer to these concepts in an organic definition of learning, that is to say, one in which the psychological processes which actually take place in the body are considered. It is well known that practically all, if not all, learning involves functional connections in the central nervous system, but including the concept of neural connections in the

definition of learning does not mean that the nervous system will have to be thought of or specifically mentioned every time a reference is made to the phenomena of learning. One can describe some of the characteristics of learning without referring to neural functions in somewhat the same way that one can discuss heredity without referring specifically to genes, and can discuss heat without mentioning molecules. But this is no argument against including neural functions, genes, and molecules in the *concepts* of learning, heredity, and heat, respectively. The body is constructed in such a way that some use of the concept of neural connections is a necessity.

Although more than one internal organic or external physical stimulus is always affecting the organism, in experimental situations the effect of all of the stimuli except one is generally equalized or held constant while the influence of the stimulus in question is carefully measured under controlled conditions. This concept of a *single* stimulus is similar to that of a *single factor*, and it is in just this sense that the concept is and for years has been most widely used in neurology, physiology, and psychology. In many learning situations, however, the *variable* stimulating pattern is not limited to a single stimulus, and we have for this reason used the expression 'stimulating processes' in the definition. The phrase 'a stimulus or a pattern or series of stimuli' could be substituted for the expression 'stimulating processes' without changing the meaning which we have intended to convey.

Although many different kinds of responses are always present in the living organism, it is impossible to observe and obtain results on all of these processes simultaneously, and some activities are always of much greater immediate interest and importance than others. The fact still remains, however, that all responses are organic pattern responses, and we have therefore used the expression 'responding processes' in the definition. The phrase 'a pattern or series of responses' could be substituted for the expression 'responding processes' without changing the meaning which we have intended to convey.

In view of the elaborate and involved construction of the body, and of the functional psychological processes which actually take place in it, the Gestalt psychologists have more to criticize in the concept of the single response than in the concept of the single stimulus. An attempt to analyze the pattern response frequently has the advantage that it leads to considerations of important related facts in neurology, physiology, and physiological psychology, but an overemphasis on the complexities of the stimulus has led a number of students away from psychology to some vague speculations about the external physical world.

We have not used the term *association* in our definition of learning because it has generally been employed in connection with conscious processes or in connection with the recall of earlier conscious experiences. Since psychology now includes more than conscious activities, the single word *association*, as a general term, is hopelessly inadequate compared with *learning, retention, and reproduction.* The conscious associative processes, however, should be included in any systematic treatment of learning, retention, and reproduction; and the term association may be employed in a convenient, although somewhat different, sense as being practically synonymous with a set or group of learned neural connections, as, for example, in speaking of an association between two words. It may be remarked that in the present discussion we are principally interested in those psychological processes which can be described in more scientific and less philosophical terms.

The part of our definition of learning following the words 'as a result of' is the *cause*, and the part preceding these words is the *result.* Although one is never directly conscious of the neural processes involved in learning, our definition of learning is so phrased that it includes learning when the subject is not conscious of the response and learning when the subject is not conscious of the stimulus. We have used the expression 'accompanying or immediately preceding psychological activities' partly because the various organic activities do not begin and cease at exactly the same time.

The word 'immediately' means a fraction of a second or a very few seconds at most, and the maximum time which should be designated is a question which will at some future time doubtless be answered more definitely by the physiological psychologists.

The causal part of the definition is designed to call attention to the actual psychological processes which take place in the body. We have not included the concepts of *remote* or *retroactive* influences, and our reasons for excluding these concepts may be shown quite clearly in connection with the following illustration. Let us suppose that an individual *dances* for the first time on Jan. 1, and that he also dances some during each of the remaining days of this month. Let us assume that he receives no instruction in dancing until Jan. 10, but that on Jan. 10 he is taught how to stand, how to hold his partner, and how to move his feet. Our illustrative subject tries to do all of these things during the remaining days of the month. Now it is obvious that the instructions received on Jan. 10 do not influence his dancing during the first nine days. When Jan. 10 arrives, the dancing of the first nine days has *ceased*, and there is nothing which can be done to it or about it. There is no sense in which the learning of *Jan. 10* can have any kind of 'retroactive' effect on the *dancing activity* of the *first nine days*. There is no way in which a natural cause acting *today* can have *its* natural result *several days in the past*. The instructions received on Jan. 10 influence the subject's dancing on Jan. 10, and probably also influence his dancing on Jan. 11–31, as a result of persisting organic conditions which act as causal agencies, however, on Jan. 11–31. There must be contiguity between organic causal activities and organic resulting activities; and we have included the expression 'accompanying or immediately preceding psychological activities' in our definition of learning to direct attention to the causal psychological processes which are present in the body. The gross failure to consider the psychological processes which are actually present in the body has led to an almost endless confusion in regard to remote forward associations, backward

associations, trace conditionings, backward conditionings, retroactive inhibitions, and 'connections' operating before, during, or after any and every or no point in time.

The term *memory*, as more or less traditionally defined by William James, means "The knowledge of an event, or fact, of which meantime we have not been thinking, with the additional consciousness that we have thought or experienced it before." This term has almost always been used in one way or another in connection with conscious processes. These conscious memory processes should be included in any systematic treatment of learning, retention, and reproduction, and one should also feel entirely free to use the words *memory, remember, recall, and recognition* when referring to the retention and reproduction of conscious processes. The most general terms, however, are learning, retention, and reproduction.

The concept of learning has been described in some detail because of the recognized importance of the concept, and our definition of learning does not seem to involve any straining or forcing of the facts. Closely related to all learning processes are the topics of retention, forgetting, and reproduction, and it now only remains to define and discuss these concepts.

Retention is the partial or complete persistence of learned connections between stimulating processes and responding processes.

It is not necessary to speak of *neural* connections in this definition of retention because we are concerned with what has been learned, and connections in the nervous system are taken for granted. We have not referred to the 'passive state' of retention or to the 'passive persistence' of connections in the nervous system partly because no neural connections can long continue in a completely unchanged form. A few elderly subjects have shown a surprisingly accurate and detailed memory for some of the episodes and events of their early lives, and the somewhat romantic accounts of these cases have given rise to an exaggerated impression in regard to the total number of life experiences that are ordinarily re-

membered. Most of what is learned is soon forgotten, and it is probable that for all practical purposes not more than one per cent of all of the conscious-verbal experiences of the early years is retained in middle life or later.

Forgetting is the partial or complete loss of learned connections between stimulating processes and responding processes.

All kinds of living tissue change with the passage of time, and the slow and rapid changes which occur in the nervous system are the principal causes of forgetting. Our concepts of learning, retention, and reproduction have been defined in such a way that all of what has been learned is retained or forgotten, what is retained is not forgotten, and what is forgotten is not retained. A failure on the reproduction test does not always mean that the material has not been learned nor that it has been learned and later forgotten.

We have not used the concept of 'inhibition' partly because this concept has for some years been in bad repute in neurophysiology. If one raises his arm, it is *logically* true that the arm does not remain still and does not move down or to the right or left, but the *process* in question is *raising the arm.* If the arm is raised, the 'inhibition of lowering the arm' is not an *organic or psychological process* which has any actual existence. These considerations apply to most of the so-called 'antagonistic' bodily systems to a greater degree than is generally supposed. If some *active* psychological process modifies previously established neural connections, this may be regarded merely as an example of an active psychological process modifying previously established neural connections which exist up to and into the present.

Reproduction is the revival of responding processes as a direct and immediate result of the action of stimulating processes, the connections between the two kinds of processes having been previously learned.

Learning may or may not be followed by retention of any appreciable duration, but if the connections are retained, they must have been learned, and if they are reproduced, they must have been learned and retained. A failure at reproduction frequently indicates that the associations have

not been formed, or that they have been established and later forgotten. A successful reproduction means that the associations have been learned and retained. The reproduction test, however, does not succeed on some trials because the conditions of the reproduction test itself are unfavorable, and not because no learning and retention have occurred. It is fairly common to assume that connections have been formed when the reproduction test gives a strong suggestion that they have not, and under ordinary conditions much material is not revived which could easily be revived under more favorable circumstances.

Recall and *recognition* have generally, but not always, been used to refer to the revival of conscious-verbal activities. The processes of conscious recall and conscious recognition are psychologically very important processes and should be included in any systematic treatment of learning, retention, and reproduction. All of the phenomena of conscious recall and recognition are included under the general term reproduction, but there are some forms of reproduction which cannot be properly designated as either recall or recognition.

The general principle on which our concepts of learning, retention, forgetting, and reproduction have been based is that the psychological processes which actually occur in the body should be taken into consideration. Some general principle is necessary, and *a* concept of learning is better than no concept at all. Just now there is a shortage of clear concepts in the psychology of learning, and it would be a worthwhile service if attention could be called to whatever limitations there may be in our principle and in our concepts.

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AN EXPERIMENTAL ISOLATION OF SOME FACTORS DETERMINING RESPONSE TO RHYTHMIC CUTANEOUS STIMULATION. III. INTERPRETATION [1]

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Certain extant theories and principles concerned with the interpretation of cutaneous phenomena are treated. The discussion is presented with reference to experimentally determined variable aspects of response to cutaneous stimulation. The doctrine of specificity (qualitative theory) versus the pattern (quantitative) theory is discussed. A biophysical interpretation of vanishing flicker and of response to vibratory stimuli, in terms of discharges in afferent nerve fibers, is considered.

The original purpose of the study was to isolate the variables involved in the response of observers to rhythmic cutaneous pressure and temperature stimulation. Several objectively controllable variables were found to be determining factors in tactual vanishing flicker (*i.e.*, response to intermittent skin stimulation as continuous). These are: (1) pressure or intensity of the intermittent stimuli; (2) time, *i.e.*, the length of time the observers had been stimulated by rhythmic stimuli; (3) the temperature of the stimuli; and (4) the adaptation of the skin to different temperatures.

The air blast method of intermittent stimulation was adopted. Allen (**5**) had used this method in an investigation of the pressure variable in 1924. Adrian, Cattell, and

[1] This study may be considered as related to the following: An experimental isolation of some factors determining response to rhythmic cutaneous stimulation. I. Frequency, pressure, and time, *J. Exper. Psychol.*, 1936, **19**, 716–31, and An experimental isolation of some factors determining response to rhythmic cutaneous stimulation. II. Temperature, *J. Exper. Psychol.* (to be published). The writer is indebted to Professor Robert D. Williams of The Ohio State University for his helpful guidance and advice.

Hoagland (**3**) used a similar technique in 1932 in an experiment designed to determine the variable aspects of bioelectric discharges in afferent cutaneous nerve fibers to rapid intermittent stimulation. Karstorf (**35**) in 1920 had used a method of intermittent stimulation by which he rhythmically stimulated cutaneous spots, which were highly tuned or sensitive to warm stimuli, by radiation.

Apparatus was constructed by which air blast stimuli of known pressure, frequency (and duration), and temperature could be delivered to the skin of the subject. Pressure was measured in mm. by means of a mercury manometer. The number of stimuli per second was quantified by the use of an electric recording device. The temperature of the stimulus was controlled and measured back of the nozzle which directed the jet of air upon the cutaneous area of the subject. The apparatus for delivering the intermittent stimuli was so constructed that the interval of time of the 'on' phase of the stimulus was the same as the 'off' phase.

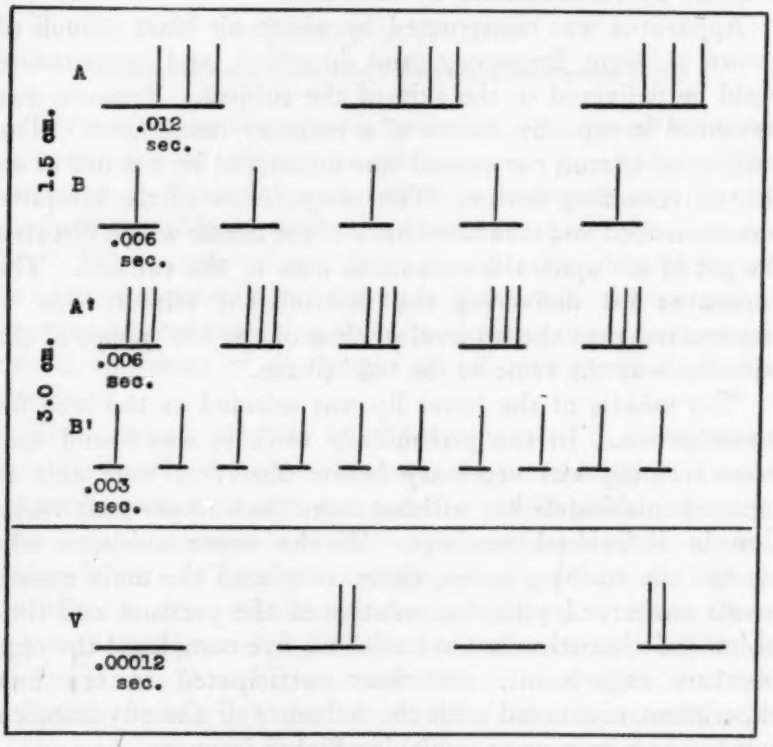
The middle of the lower lip was selected as the area for investigation. In the preliminary work it was found that some training was necessary before observers were able to report vanishing flicker without more than 10 per cent variation in individual readings. Of the seven subjects who started the training series, three completed the main experiments concerned with the isolation of the pressure and time of intermittent stimulation variables, five completed the temperature experiment, and four participated in the final experiment concerned with the influence of the physiological temperature zero upon vanishing flicker frequency.[2]

Specificity Versus Pattern Theory

There are two major types of extant theories of interpretation of cutaneous phenomena. One of these may be called the specificity, or qualitative theory; the other is more recent historically and may be referred to as the pattern or quantitative theory.

[2] The experimental method and results have been reported in some detail in another place, cited in the preceding footnote, and are not repeated here. For the purpose of the present study the findings essential to the interpretation are presented in the upper part of the figure and in the text on pp. 71 ff.

The doctrine of specificity, perhaps first suggested by J. Müller in 1838, has been extended to account for different qualities in the sense departments of vision and audition by Helmholtz and by Du Bois-Raymond. It has been modified and extended to the cutaneous realm, particularly by von Frey and by Head.



Shortly after the discovery of the punctate distribution of the sensitive spots on the skin in 1884–85 independently by Blix, Goldscheider, and Donaldson, von Frey (1895, 46) formulated his correlation of the temperature and touch spots with specific end-organs. He stated (with reservations) on logical grounds that Krause end-bulbs mediate cold, Ruffini cylinders warm, and Meissner corpuscles subtend touch qualities. Some modifications of the specificity doctrine maintain separate and differentiated nerve fibers for each

different receptor. Others are content to leave the specificity in the receptor.

Nafe (**39**) maintains that different patterns of nervous activity are set up in similar, undifferentiated nerve fibers, for different patterned conditions of stimulation. All cutaneous experiences are phenomenal patterns correlated with (1) Frequency of nerve impulses, which has been shown by the electrophysiologists to be directly related to intensity of stimulation; (2) Duration of nervous impulses which is proportional to the duration of change set up in the receptors and the length of time such change continues, and is correlated with felt duration; (3) Density of discharge: if only a few fibers are activated the patterns of impulses and the felt experience will be different from a condition in which many are functioning; (4) The size of area stimulated. This varies with both the size of the stimulus and its intensity, and is correlated with felt extensity.

According to this view there is no specificity in nerve fibers—each nerve will subtend any quality according to the pattern of stimulation. This equipotentiality of nerve tissue places the differentiating properties upon the pattern of excitation itself, rather than upon qualitative considerations of nerve fibers.

The specificity views have little experimental evidence to support them. The histological investigation made by Donaldson (**18**), Häggqvist (**28**), Strughold and Karbe (**43**), Dallenbach (**17**), and Pendleton (**40**), in general do not support the specificity theory. Heiser found that, "In general the number of temperature responses within a given area of skin increases with an increase in stimulus temperature" (**31**). This study of Heiser and also that of Baron and Bentley (**10**) indicate that the finding of a punctate distribution of sensitive spots is at least in part conditioned by the stimulus values used.

We are not necessarily to conclude from these studies that the sensory mechanism is not distributed in a punctate way, but that the pattern theory (that the pattern of the stimulus, which includes its intensity, extensity, and duration,

sets up a pattern of nervous discharges with which felt experience is correlated) is perhaps a more apt interpretation than qualitative specificity of stimulus—receptor—nerve fiber —phenomenal experience. Experience is correlated with differences in neurological activity which may be expressed quantitatively, according to Nafe (39) and Hoagland (34). Furthermore, it is felt that the pattern theory lends itself more readily to the problem of vanishing flicker than does the specificity doctrine.

A Biophysical Interpretation of Vanishing Flicker

The factual information which now exists concerning the properties of discharges in sensory fibers has been gleaned from experimentation upon the lower animals and under highly artificial experimental conditions. Certain pertinent facts of a general type are known concerning the variable aspects of nervous activity. Some of these facts are presented below.

1. A difference exists between phasic and postural sensory mechanisms. Phasic mechanisms are cutaneous; postural mechanisms are muscle, or proprioceptive. Postural mechanisms adapt themselves very slowly, *i.e.*, nerve impulses continue during prolonged stimulation. Phasic mechanisms are characterized by their rapid decremental adaptation rate. Persistent sensory discharge is not present in phasic mechanisms. The time relations found by Adrian and others are markedly comparable to those found in our flicker experiment, as will presently be shown. The work of Adrian, Cattell, and Hoagland dealing with response to intermittent stimulation indicates that the mechanism of phasic discharge is to be found in the epidermis of the frog since, "If the epidermis is completely removed and the corium exposed the normal tactile response does not return . . ." (3, p. 389).

2. An increase in the intensity of the stimulus (*i.e.*, amount of increase of intensity per unit time), applied to a cutaneous area, brings about an increase in frequency of discharge in each individual nerve fiber (Adrian).

3. An increase in intensity results in an increase in adapta-

tion time for each nerve fiber of phasic mechanisms (Adrian).

4. Moderate intensity of stimulation does not excite all of the fibers which supply a given area (Erlanger, Bishop, and Gasser, **21**) and of the fibers that do respond some react more quickly than others (Bazett and McGlone, **8**).

5. Increase in pressure results in an increased tendency for the different individual fibers which are involved to discharge together (Adrian).

6. "Some mammalian nerves tend to show a synchronized discharge where all fibers are discharging at high frequencies and together so that a quick succession of volleys is sent over the nerve in which every fiber may be involved" (Nafe, **39**, p. 1040). Adrian has made a number of records of nerve impulses produced by moderate pressure on the toe pad of the cat. Duration of impulses is shorter in warm blooded animals or when the preparation is kept at the body temperature of a warm blooded animal (Adrian, **2**, p. 58).

Other evidence from studies of the lateral line receptor organs of *Ameiurus* indicates the possibility of a one-to-one relation between rapid intermittent stimuli and response in peripheral afferent fibers. Hoagland states, "In general the asynchronous spontaneously discharging lateral line organs of catfish and trout appear to respond to vibratory stimuli from tuning forks by getting into phase and beating synchronously at frequencies ranging from 20 to 70 per second. This frequency of beating seems to be independent of the frequency of the tuning fork for frequencies used of 100, 200, and 250 dvs./second" (**34**, p. 67).

Two assumptions are necessary if we are to interpret flicker responses in terms of the results of the electrophysiologists. These are concerned with the nervous correlate of flicker phenomena. The interpretation given below assumes first that either only one single nerve fiber is activated by the air blast method of stimulation, or that a number of fibers are acting in synchrony, as a unit. From the known facts concerning nerve action either of these alternative assumptions is possible. Secondly it must be assumed that the nervous activity of the frog and cat which were measured by these workers is directly comparable to that of the human.

Neither of these assumptions may be warranted. Still certain marked similarities exist between the two sets of data so that it is felt that an interpretation in terms of the findings of Adrian, Cattell and Hoagland, who used the intermittent air blast method as stimulus to excite activity in the frog's nerve, may be justified.

A sketch of six of the general findings of the electrophysiologists has been presented. Certain other factual data somewhat more immediately pertinent to the material of the present study have been reported in papers by E. D. Adrian, McKeen Cattell, and H. Hoagland (**3**), and by Cattell and Hoagland (**16**).

In both of these studies the experimenters were observing response of single afferent nerve fibers to intermittent stimulations of the skin of the frog. One antidromic branch of a fiber was used to sample the discharge which passed up the other branch. They made observations on the maximum frequency of sensory impulses under different frequencies of intermittent stimulation and on the reactions of the animal to sensory discharges of high frequency. They used the air blast method of stimulation since "Preliminary trials showed the latter to be an adequate stimulus to the end-organs of the frog's skin, the rate of discharge following the frequency of interruption over a wide range" (**3**, p. 381). They used pressures of from 500 to 800 mm. mercury, and revolved a sectored disc in front of the nozzle for the purpose of interrupting the air jet. The frequency of stimuli used was as high as 400 per second. These investigators painted the frog's skin with liquid paraffin to prevent drying.

The experimenters expected, due to the rapid adaptation rate of the phasic mechanisms with which their study was concerned, that a continuous air blast would call forth only a few nerve impulses. They found that when the air was applied instantaneously by ". . . quick removal of a screen from in front of the air jet the number of impulses was reduced to three or less, and usually to only one" (**3**, p. 386). This finding indicates that nerve impulses are set up (at least under the conditions of their experiment) ". . . only during

the actual period of movement of the skin although the air pressure is great enough to cause a visible pitting of the skin to a depth of several millimeters during its application." Adrian had concluded from the results of his former work (**2**, p. 64) that cutaneous mechanisms adapt slowly when compared to the adaptation rate of nerve fiber, but it is now known that phasic cutaneous mechanisms adapt as rapidly as the nerve fiber itself. Adrian, Cattell and Hoagland maintain that such mechanisms adapt quickly and that their discharges resulting from such mechanical stimulation as an air blast ". . . are comparable with those of a nerve fiber to an electric current. . . . In the cat the sensory endings activated by movement of the hairs behave in the same way; *i.e.*, impulses are only set up during actual movement of hairs and a very slow movement may be without effect" (**3**, p. 387). They found that when a slotted disc is rotated in front of the air jet so that the stimuli are intermittent, a series of nervous impulses are recorded which follow the frequency of application of the stimulus. The maximum number of impulses which may be so synchronized (in step) with the intermittent stimuli was found to be as high as 250 to 300 per sec. Hoagland says with reference to pressure and pain stimuli that "Encapsulated endings are rare and are so distributed as to make it seem very unlikely that they are specific and exclusive receptors for either of the two classes of stimuli" (**32**). He holds that the burden of specificity or sorting of quantitatively different patterns of impulses is placed upon the central nervous system.

From the above considerations we may state that in human afferent nerve fibers (granting for the present the two necessary assumptions mentioned above):

1. When the human subject's cutaneous surface is stimulated by intermittent stimuli of slow frequencies and high intensities, more than one impulse will be discharged by the phasic end-mechanism (whether free nerve endings or ends of an encapsulated kind) as a result of each stimulation. The pattern of nervous impulses in an afferent fiber (or fibers) will be temporally asymmetrical (*i.e.*, out of step or not syn-

chronized with the stimulus pattern). Such asymmetry may be correlated on the psychological or response side with felt discreteness or discontinuity of intermittent stimuli or flicker.

2. When the subject's skin is stimulated by intermittent stimuli of high frequencies and high intensities, only one impulse will be discharged by the phasic end-mechanisms for each stimulus and the pattern of impulses which are thus synchronized with each stimulus will be symmetrical—*i.e.*, there will be a constant or one to one relation between the stimulus pattern and the pattern of nerve impulses. The pattern of symmetry may be correlated on the psychological side with continuity, the subject reports continuity of pressure.

3. When the frequency is increased from slow to rapid intermittent stimulation and the pressure is held constant, a frequency will be reached at which patterned impulses will correspond with the temporal pattern of the stimulus, *i.e.*, one impulse resulting from each stimulus. This frequency may be correlated with the response of vanishing flicker.

4. When the subject is stimulated with intermittent frequencies of lower intensity, the frequency must be lower (duration longer) for reports of just vanishing flicker (symmetry). Frequency of nerve impulses is known to increase with increase in intensity of the stimulus.

5. With low pressure and very low frequency, the stimulus would be slow enough to elicit more than one impulse during its 'on' phase, so that the pattern of impulses would be asymmetrical (*i.e.*, below the phenomenal or response threshold for vanishing flicker).

6. When frequency of any moderate pressure of intermittent stimulation is increased beyond the first frequency of patterned symmetry at which the subject reports vanishing flicker, the impulses (probably) keep in step with the increase in frequency up to the limit of the refractory period of the fiber (or fibers) involved, at which very high frequency it is known that some of the impulses begin to drop out, the result being patterned disparate asymmetry. This relation might be correlated on the psychological side with vibratory discrimination.

The last principle is an inference which may be of value in an interpretation of cutaneous vibratory phenomena (rapid intermittent tactual stimulation) on the same basic principles as are involved in the interpretation of vanishing flicker phenomena. This possibility will be treated several pages further on. Regarding the first five statements, the time relations found in the present study are schematically presented in Fig. 1, parts *A* and *B*; and *A'* and *B'*.

Parts *A* and *B* show the probable nerve activity during intermittent air blast stimulation with pressure of 1.5 cm. The black horizontal lines represent the 'on' phases of the stimulus, the interspaces between them show the 'off' phases. The vertical lines represent hypothetical individual nerve impulses resulting from the stimulation. These show in a similar manner on the photographic records of the electrophysiologists and are called 'potential spikes' by them. Each spike represents a recorded change in the bioelectrical potential which accompanies the single nervous discharge, tentatively described as a propagated wave of negativity.

There is known to exist a temporal initial lag in discharge after the stimulus is applied so that the spike might appear when the stimulus is not 'on,' but for our purpose the spike is represented as occurring during the 'on' phase.

Section *A* in the figure represents a condition of patterned asymmetry, *i.e.*, the spikes are not synchronized with the stimulus pattern. At 1.5 cm. pressure in the present investigation all subjects reported discontinuity, *i.e.*, they discriminated discrete impacts, at durations of .012 sec. At the same pressure when the frequency was increased so that each stimulus was 'on' for a duration of approximately 6 milliseconds (the average vanishing flicker frequency found in our experiment for this pressure) all subjects reported continuity or vanishing flicker. The probable nervous condition correlated with this frequency and pressure value is patterned synchronized symmetry of nervous impulses as shown diagrammatically in section *B* of the figure.

In section *A'* is represented a condition of disproportionate or disparate non-synchronized patterned impulses resulting

from a stronger stimulus intensity of 3.0 cm. applied at the same frequency as that shown in section *B*. Under this condition all subjects reported discontinuity. For the higher pressure values vanishing flicker is reported at higher frequencies (shorter durations). Section *B'* shows the longest appropriate duration for vanishing flicker—approximately .003 sec. At this pressure and duration the impulses are shown as synchronized and in step with the stimulus presentations. On the psychological side this condition is correlated with discriminated continuity.

The lower part of the figure shows a possible interpretation of the phenomena of cutaneous discrimination of so-called vibratory stimuli. Before considering this interpretation a brief survey of the present status of the problem of tactual-vibratory discrimination is presented in its relation to cutaneous phenomena.

Gault and his collaborators have devised a method of intermittent tactual stimulation in which the sense of touch is used as an aid in the education of the deaf. The apparatus used for stimulation is called a teletactor (**27**) and consists of a microphone, amplifier, and receiver or vibrator which is placed upon the observer's cutaneous surface (the finger tip has been found by Gault to be most sensitive and convenient for this purpose).

The upper frequency limits have been placed by several experimenters at from 1300 to 2700 d.v. per sec. and Gault has recorded discriminations as high as 8192 d.v. per sec. when sufficient amplification is used, according to his co-worker Goodfellow (**6**, p. 387). It is maintained that, with the proper amplification, deaf observers would be able to discriminate through touch mechanisms ". . . all pitches in the same intensity relation as the normal person does through hearing."

Gault recognizes the possibility of tactual sensitivity alone being capable of such discriminations but is at present apparently inclined to the opinion that there is a special sense of vibration in terms of which his data will eventually be interpreted. He states, "There is a deal of evidence that

a deeper sense of vibration is involved with that of touch in these experiments" (**24**). We feel that it is perhaps not necessary to postulate a deeper vibratory sense.

Let us consider the possibility of eliciting nerve impulses by stimulating the skin with a frequency of 4000 stimuli per sec. The vibrator or teletactor method is not directly comparable to the air blast method of stimulating since the vibrator is a hard stimulus, and also since the 'off' phase of the stimulus may not be equal to the 'on' phase. Yet certain relationships are evident. At this frequency of 4000 stimuli per sec. the duration of each separate stimulus would be approximately .00012 sec. This frequency would perhaps result in a non-synchronized asymmetrical pattern of nerve impulses, since it has been found that if the frequency is increased beyond a certain value the series of nerve impulses would probably be out of step with the evenly spaced intermittent stimulus pattern. Adrian, Cattell and Hoagland maintain that with repeated stimuli "of short duration produced by the rotation of the slotted disc in front of the air jet a series of antidromic impulses are set up in a single nerve fiber preparation, and these may follow the rate of interruption for long periods if the frequency is not too high. When it is very high, the frequency of discharge does not keep pace with that of the stimulation except at the outset" (**3**, p. 387). Under such conditions the psychological correlate would be discriminated discontinuity. It is not impossible that such non-synchronized patterns are set up by the teletactor method of stimulation. Part V of Fig. 3 shows this possible relation. The potential spikes are shown to be disproportionate to the stimulating frequency.

If response to rapid intermittent stimulation may be interpreted in this way we may conclude (1) that there is no specific vibratory mechanism in the skin which subtends discrimination to vibratory stimuli; (2) that the explanation ultimately will be in terms of quantitative patterns of nervous activity in afferent fibers with specificity and sorting a function of the central nervous system.

Summary

A biophysical interpretation of vanishing flicker has been presented in terms of the known function of peripheral nerve elements during rhythmic stimulation. The psychological response of vanished flicker is dependent upon certain objective stimulus conditions which have been isolated. Certain nervous functions are correlated with these conditions.

On the basis of the present knowledge of the variable aspects of nervous discharges (Adrian and his co-workers) flicker may be said to be probably correlated on the neurological side with the elicitation of more than one nervous impulse per stimulation, and vanished flicker with a one-to-one relation between frequency of stimulation and frequency of discharges in peripheral afferent nerve fibers. Other variable aspects of discharges correspond with the variable aspects of response to rhythmic stimulation, *viz.*, intensity varies directly with frequency of discharges; adaptation time varies inversely with intensity of stimulation; and the fact that at certain wide ranges of frequency of intermittent stimulation the nervous discharges are known (Hoagland) to keep in step with intermittent stimulus presentations.

No specific classification of epicritic-protopathic or superficial-deep sensitivity appears to be necessary for interpreting the results. The data are perhaps more readily interpreted in terms of the quantitative pattern theory of cutaneous phenomena than on the basis of the qualitative, peripheral specificity doctrine. The data may be interpreted in terms of the neurological activity which is correlated with the influence of the variables of pressure and adaptation time upon vanishing flicker frequency.

The phenomenon of vibratory sensitivity may be interpreted in terms of asymmetrical discharges in afferent nerve fibers.

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AN EXAMINATION OF SOME PHASES OF SPACE PERCEPTION

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At the present moment psychology carries an unnecessary burden. We refer specifically to the obvious confusion which is directly attendant upon the theoretical treatment of certain phases of space perception. It is disconcerting to pick up two elementary texts in psychology and discover two diametrically opposed statements concerning the nature of direct experimental data upon a particular problem. When such opposition concerns fundamental principles, the matter assumes an aspect sufficiently serious as to demand recognition.

Two experimental studies made by Stratton on visual perception, together with their subsequent descriptions and interpretations by various psychologists, have definitely contributed to the current confusion. Stratton's studies dealt with the nature of the monocular field under conditions in which the stimulus pattern (by employment of lenses) was 'inverted' on the retina. Although these reports appeared forty years ago, current text books of psychology still contain strikingly contradictory statements concerning the nature of the perceptual observations and the action patterns which emerged while the lenses were being worn, as well as following their removal. Yet elaborate explanatory theories concerning the spatial characteristics of observable objects have been built upon the results of Stratton's studies. The type of confusion which appears in this particular case cannot be lightly dismissed. Forty years offer ample time to straighten out a factual matter. The confusion can actually be eliminated if proper emphasis is placed upon experimental report.

We restrict our present treatment of space perception with 'inverted pattern' to three phases. We shall accordingly attempt (1) to explain the confusion attendant upon this

problem, (2) to show from his original reports that Stratton did not actually *see* objects in his perceptual field as being upright, and (3) to evaluate Stratton's results in terms of Ewert's data. Stratton's experimental reports were published in 1896 and 1897. References to these two studies occur widely in the psychological literature, particularly in our elementary texts. We wish now to consider briefly the way in which some psychologists deal with this matter.

Writing of the effects of 'retinal inversion' upon Stratton's perceptual field, Woodworth remarks:[1] "The lenses reversed right and left as well as up and down. The field of view was not distorted as a picture, and the eye and head movements of looking towards an object were not disturbed. But the relation of the visual field to hand, foot and body movements was reversed, with great resulting disturbance of movement and orientation. At first, everything looked upside down and reversed right and left, but in the course of the week, a new system of visual signs and spatial meanings was built up, *and things looked right side up again*" (**13**, 1929 ed., p. 401). Five years later (1934 revision), Woodworth completely reverses this position to say: "By the end of the week, the subject was able to walk freely about the house, and to perform all sorts of manual operations, and his behavior in familiar surroundings became almost normal, though the *field of view did not appear right side up*" (**14**, p. 386). Which of these two positions may we safely accept? Which accords most adequately with the experimental results as reported by Stratton? Did Stratton, while wearing the lenses which rotated the stimulus pattern through 180°, actually *perceive* the objects of his perceptual field as being 'right side up'?

Do other psychologists offer a basis for a satisfactory resolution of this matter? The position of Carr, for example, stands opposed to that of Woodworth. After remarking that motor disability gradually disappeared while the lenses were worn, he states that Stratton "was finally able to respond correctly to visual objects in a perfectly normal manner. The subject thus acquired a new system of visual-motor habits

[1] All italics ours.

that were adapted to the novel optical conditions. Coincident with this motor recovery, the illusory location of visual objects disappeared. An object was now seen in the place where it was heard and felt to be. Conceptual space was also reorganized in a manner adapted to the new conditions, *and the whole world once more seemed perfectly natural and normal*" (**2**, p. 138). A like interpretation is given by Murphy. "At first, the world is actually seen as if in a mirror, but within a few days the process of dealing with the world with our muscles in a direct way brings about a new set of habits. If we reach for an object immediately after putting on the lenses, and find that the hand has gone too far to the left, we reach to the right, or what we think is the right, with the result that we make an even worse error. Yet after practice the subject not only ceases to make faulty moves—that is, builds up a system of movements which are well articulated with visual stimuli acting on him—but he actually *sees* the world right again" (**8**, p. 188). From another source we learn that Stratton was forced to reconstruct his older motor habits. As a result, "The usual and the inverted visual fields seemed equally normal when the new habits became fully established. In other words, Stratton *ceased to be conscious of the difference between the two*" (**9**, pp. 130–1). It is unnecessary to continue our citation. These sources are sufficient to reveal clearly the urgent need of clarification. Two points are involved here which we shall shortly relate to Stratton's original reports, *namely*, (1) the nature of the spatial relationships with respect to 'uprightness' and (2) the matter of 'naturalness, normality, articulation or consciousness of differences.' We shall in part examine the nature of Stratton's reports with respect to these two points.

Supplementing what has been said above regarding Stratton's *action patterns* (as distinct from his perceptions) while the lenses were being worn, we note two other interpretations. Dashiell points out that since the retinal images were revolved 180 degrees, the hands and feet and floor below fell upon the retina as if from above, and things located above fell upon it as if from below. Under these conditions, "all visually

guided movements of the body were at first awkward, uncertain, full of surprises. When an object on the right (stimulating him as if on the left) aroused a movement to the left, the error led to inhibition and movement to the right. This process of readjusting through much trial and error continued with increasing success until on the third day fairly well organized eye-hand and eye-foot coördinations appeared" (4, p. 408). From another source we learn that "at first, movements in response to seen objects were entirely false, the subject reached in the wrong direction for any seen object, and ran into anything that he tried to avoid. Movements guided by the eye were laborious and nerve racking, and had to be performed, either by patient calculation or else by mere trial and error. By the end of the week, the subject was able to walk freely about the house, and to perform all sorts of manual operations. Early in the experiment, after the subject had come to see what was before him right side up, he still could not think of what lay outside of his immediate field of view as belonging in the new system, and did not know in the least where to look for parts of the room that he did not actually see; but by the end of the week the new system extended to include objects not actually in sight, so that the subject's behavior in familiar surroundings became almost normal" (13, pp. 401–2).

Let us now consider the situation with respect to what Stratton saw and did when he removed the lenses. One psychologist states that when Stratton "removed the lenses, at the end of the week, the first effect was one of pleased surprise at finding himself back in his old visual space; there was considerable bewilderment and movements were false again, since they continued to follow the new system. After a few hours, however, the old system was fully reestablished. The *old system had not been unlearned or damaged* by the week's drill with the reversed fields. It is clear, from the experiment, that erect vision is just a matter of integrating visual data with other sense data and with bodily movement" (13, p. 402). In another elementary text, however, we find an exactly opposite position. We are told that "upon removal of the

glasses *normal coördinations were restored instantaneously.* No better example need be sought to demonstrate the fact that perceiving is nothing more or less than a form of habitual response. There is no mystery about the fact that the visual image on the retina is inverted in relation to the object from which it is projected: 'seeing' a thing right side up, or upside down, or inside out, is a matter of what the person is used to reacting to. Whatever he had adequate reactions toward, is taken to be the 'real' thing" (**4**, pp. 408–9). The position of another psychologist, who emphasizes the importance of learning, is to be considered in terms of the above statement of Woodworth's. "With the removal of the lenses," we read, "the subject attempts to utilize the new system of habits and failure and confusion naturally result. As a consequence, the subject is compelled to *relearn his old system of habits and normal vision is once more secured.* It is thus obvious that the Stratton experiment involves no reconstruction or alteration of tactual and auditory space. It is the visual system that is disrupted and then reorganized so as to conform to touch and hearing" (**2**, p. 141). Murphy remarks that "after taking off the lenses, he again makes his adjustments the wrong way around; but quickly gets back to his earlier pre-experimental kind of orientation" (**8**, p. 188).

This brief examination of a few (possibly representative) sources clearly reveals in part a scientifically disturbing state of affairs regarding the matter of experimental observation and action while the lenses were worn and after they were removed. We do not wish to be guilty either of attacking a straw man or of laboring on a point. We believe that a serious degree of confusion and contradiction exists and that it may be removed. We suggest a way of removing a situation in which we are told (*a*) that things were seen right side up and (*b*) that they were *not* seen right side up; (*a*) that the whole world seemed perfectly natural and normal (Stratton "*ceased to be conscious of differences*") and (*b*) that behavior in *familiar* surroundings became *almost* normal; (*a*) that upon removal of the lenses normal coördinations were instantly restored and (*b*) that there was considerable bewilderment and

movements were false again; finally, (*a*) that the old system 'had not been unlearned' and (*b*) that the subject is compelled 'to relearn his old system.'

In order to evaluate adequately this particular problem which, as we envisage it, includes the several positions briefly indicated above, we must necessarily turn to Stratton's original reports. They must unquestionably stand here as a basic foundation. We shall accordingly show, from his own reports, that Stratton did not actually *perceive* the environmental objects around him to be 'upright.' Finally, we must attempt to understand Stratton's position upon this problem of space perception by relating his data to those obtained by Ewert under more carefully controlled experimental conditions involving several subjects (Stratton was his own subject) and extending over a much longer period of experimental observation.

An understanding of this problem rests upon a recognition that no small amount of the confusion and contradiction to be found in the literature which deals with this problem must be directly attributed to the descriptive nature of Stratton's original experimental reports. It is an extremely difficult task to strip his accounts sufficiently of their gross ambiguities as to make them straightforward and understandable. Not only are the descriptions obscure, but no quantitative data of any sort are presented from which deductions can safely be made. Of his first report (based upon 21½ hours observation —1896 study—with a lens over one eye), we find this very significant statement: "As to the relation of the visual field to the observer, the *feeling* that the field was *upside down* remained in general throughout the experiment. At times, however, there were peculiar variations in this feeling according to the mental attitude of the observer toward the present scene. If the *attention* was directed *mainly inward*, and things were viewed only in *indirect attention*, they seemed clearly to be inverted. But when, on the other hand, *full attention* was given to the *outer objects*, these *frequently* seemed to be in normal position, and whatever there was of abnormality seemed to lie in myself, as if head and shoulders were inverted

and I were viewing objects from that position, as boys sometimes do from between their legs. At other times the inversion *seemed confined* to the face or eyes alone" (**10**, p. 616). We are unable to reach a satisfactory understanding of what is included here in the term 'feeling' or in the phrases 'directing the attention mainly inwardly and outwardly,' and 'outer objects.' Possibly, if Stratton's descriptions had dealt less with 'feelings and attitudes' and more with specific observable characteristics of the perceptual field, there would be less confusion and uncertainty.

Our lack of such specific understanding forces us to question any form of 'attention' which is sufficiently powerful to enable an individual (through a change in the direction of his attention) *to turn environmental objects in his perceptual field upside down or right side up without the slightest change being made in the stimulus conditions.* Here, indeed, is a significant position, especially for those psychologists (and the writer considers himself among them) who stress the importance of stimulus as a causal agent. According to Stratton, the relations of the *stimulus* and *end-organs* (retina) lack any causal significance as far as those spatial characteristics of perceptual objects which we consider here are concerned. The stimulus pattern may accordingly be imposed upon the retina *at any angle* (30°, 90°, 180°, 270°). Yet the organism, as a direct result of tactual and kinæsthetic experiences, will come finally to perceive an 'upright' object. Stratton implies that this particular observable property—'uprightness'—of the objects of the visual field is a direct resultant of 'experience.' The perception of upright position with inverted retinal pattern accordingly represents no inherent function of man's visual mechanism. In order to hold that it does, it would be necessary to assume an unnatural condition, if for no other reason than it would demand a 'reinversion' of physiological processes in order to account for 'uprightness.' That is to say, it is not 'natural' for an 'inverted' image to be accompanied by an 'upright' object. The course of reasoning, he holds, must run as follows: The retinal *image* and the *object* perceived are causally related. This relation implies

that they must necessarily correspond or harmonize, that is to say, the perceived object *as pattern* must actually correspond to retinal image *as pattern*. 'Experience,' serving in some undefinable manner as causal agent, steps forward to produce this harmonious relationship. Experience thus removes the unnatural condition (Stratton) of 'inverted image' and 'upright object.' In conformity with this position, Stratton remarks that "We have no reason to suppose that there even is a reversal or transposition of directions in the visual process" (11, p. 185). He then theorizes to say that "The inverse relation is a psychologically non-significant accompaniment of the peculiar lens-arrangement of the eye, and would disappear could we but change the eye in that regard" (11, p. 187). Such inversion is completely nullified, according to Stratton's views, through the influence of touch upon the spatial properties of visually perceived objects. As 'experience' works, the transitory illusion (as in this case) of seeing an object upside down is gradually overcome. This readjustment of the perceptual field must necessarily occur because touch gives *true* knowledge about the object. According to Stratton, touch really conforms to reality, but vision does not.

We are definitely troubled here, furthermore, by a lack of knowledge about (1) the number of times that the environmental objects were seen either 'normally or abnormally.' It would be instructive to be able not only to place the 'frequent' appearances (Stratton's report) within the day's total period of observation, but to understand something concerning the specific manner of their phenomenal appearance or emergence in the perceptual field. Did this spatial characteristic of environmental objects suddenly emerge? Did the perceptual field suddenly swing around from an 'inverted' to an 'upright' relationship, and as suddenly turn upside down again? Or, did all such changes occur at a non-observational level so that Stratton simply discovered the field to show certain spatial characteristics at one time and other characteristics at another time? Again, we would be definitely aided by knowing whether the perceptual field changed in part or as a whole —did some objects change with respect to their spatial prop-

erties while others did not? When uprightness (of objects) finally appeared in the field, did it then persist for several hours without reversals occurring? Again, (2) we lack any knowledge concerning the nature of those *particular objects* (if the field changed in part) which, at any given moment, were seen either normally or abnormally. We do know, however, as Stratton remarks, that *familiar* environmental objects were different from unfamiliar. He could accordingly act (move) toward the former sort more readily than the latter, a fact which the thoughtful student must find to be very significant. How could familiarity possibly facilitate behavior patterns? If the effects of visual experience were actually uniform throughout; that is to say, if the perceptual field were quite homogeneous (in the sense that the objects in a particular observed situation could serve equi-potentially in determining what was seen and done), behavorial 'adjustment' to various objects, as in a room, should unquestionably have been uniformly effective throughout. Such, however, did not seem to be the case. What, then, is specifically implied by such differential behavioral adjustment in terms of this one factor of object-familiarity? Since Straton does not say, perhaps we may be permitted to suggest the following as being typical: With the lenses in place, the experimenter walks into a familiar room. He specifically understands, for instance, that his desk stands at the right of the door and that his chair is placed to the left of the desk, as he faces the latter upon entering the room. He can accordingly turn right toward the desk with perhaps as much ease *as he could in complete darkness*. Moreover, since the observer fully understands the major effects of the lenses upon the spatial characteristics of his perceptual field, he can actually walk without much trouble toward the chair which he now recognizes to be upon the right of the *object* (desk)—upside down and with left and right reversed—that he perceives ahead of him.

But the matter is different when the experimenter suddenly encounters an *unfamiliar* object in his perceptual field. It is now necessary to pause while he actually determines, in terms of his understanding of the reversal effects, the particular

direction in which he must move to deal adequately with the unfamiliar object. Stratton unquestionably established his action patterns with respect to objects in part upon his *understanding* of spatial relations under these specific conditions (lenses). Finally, it would be instructive to know something regarding the nature of those determining conditions, aside from 'attention,' under which environmental objects were seen as being either in a normal or in an abnormal position. What, if any, were the discoverable causal factors which lead at one moment to reported 'feelings of normality' and at another moment to 'feelings of abnormality'? For instance, did a difference in overt behavior—manipulating actions—serve as one condition here; that is to say, was there any casual connection between amount of activity (practice) toward a particular environmental and the degree of 'felt' normality? We are simply told that when full attention was given to environmental objects they 'frequently seemed to be in normal position.'

A further problem confronts us. When, as under the influence of 'attention,' an environmental object had *once appeared* as being 'upright,' why did this observed property fail to persist? If new *habits* (tactual and kinæsthetic) had actually been established with respect to objects, as is definitely implied in Stratton's study, to produce a condition of perceptual normality, *why did these habits not continue?* Why did not uprightness then persist as long as the lenses were worn? Stratton's results offer no answer to such questions. We are directly led, therefore, to question the existence of any such habits. It is impossible to understand a habit that works at one moment to produce a particular observational effect as in this case, but that does not work a moment later although no known change occurs in the experimental situation. Lacking specific and necessary information bearing upon the emergence, persistence and disappearance of uprightness, it is quite impossible to derive either an adequate understanding or a reliable evaluation of Stratton's reports. We may, however, be fairly sure about one point in this particular (1896) study, *namely*, "the feeling that the field was upside down remained in general throughout the experiment."

Disregarding whatever possible effects which hereditary mechanisms might exert to produce continuous inversion of the perceptual field while lenses are worn, it is surprising that it would be assumed that a period of 21½ hours of observation could so greatly affect perceptual patterns as to lend credence to 'experiential' explanations of this particular type. Stratton himself states that the time was "altogether too short from which to expect very pronounced results in undoing a life-long habit of interpreting visual signs, but which, nevertheless, gave interesting indications of what would result if such an experiment were considerably extended" (**10**, p. 613). But his longer (1897) study actually failed to reveal results significantly different from those obtained in 1896. Moreover, after 14 days of continuous wearing, Ewert's subjects did not observe the slightest change in the spatial properties of the perceptual field. This also held for other properties such as depth and movement. Ewert's subjects did, however, learn (14 days) to act quite efficiently with respect to environmental objects, as was shown by daily scores on experiments such as card sorting.

In addition to the factor of descriptive ambiguity, a certain amount of the confusion and contradiction found in our literature concerning this particular problem is directly attributable in part to the quality of Stratton's original reports and in part to a confusion of fact and explanation. Some psychologists seemingly refer to his 21 hours of observation (1896) while others refer to his 87 hours (1897). To obtain a broader understanding here it is necessary, therefore, to glance briefly at these two reports in terms of the nature of the observer's perceptual field and action patterns during the periods in which lenses were worn and following their removal. We turn now to the original reports.

Stratton's basic theoretical position upon this matter of 'uprightness' of object without 'inversion' of retinal image is revealed in his comments upon his first study. His position must be regarded in terms of his *unusual interpretation of the nature of the retinal image and its place in the total series of factors involved in visual perception.* The image is not re-

garded by Stratton as it commonly is in science, *namely*, as a spatially distributed pattern of organic changes induced by environmental energy. Retinal image, for him, is an actual part of his visual field. We accordingly note the following statement: "It is sometimes said that one never has an experience of his own retinal image. This is perhaps strictly true, and yet it is often misleading. . . . The fact is, *the retinal image is, by representation, made a part of my experience, just as all things which I represent become thereby parts of my experience, even though I do not directly perceive them.* . . . It is visualized, or otherwise represented, in definite spatial relation to those parts of the world which I see, and thus becomes an *integral part of my larger world of visual* and visualized experience, built out beyond and in between the objects of actual sight. My brain, for instance, becomes a part of my visual world because I assign it a definite position within the visual total. . . . *Likewise my retinal image is an integral part of my visual world. Its place is within my visual total, and its position and direction are determined only by making use of the directions of reference within that total*" (11, p. 183).

The above statements will possibly aid in the following difficult bit of speculation by Stratton upon the significance of his study. These comments reveal clearly the particular manner in which Stratton secures an 'upright' object with an 'upright' retinal image. "Suppose that the partial reharmonization of my experience had given place to a complete harmony of tactual and visual perceptions, I would then feel and see my body unreservedly in its new place in the visual field, and in the same relation to the new objects around my body, as existed between my body and the surrounding objects in the older experience, *viz.*, my feet on the ground, my head toward the sky, *etc.* The proper localization of my retinal image according to the laws of experience, would now produce no such contradiction. I could localize the image upright with respect both to my body and to the objects pictured in my image. The inverse relation between my retinal image and the objects perceived would here have disappeared" (11, p. 185).

Let us now glance at the 1897 report as found on the third day. At this time, after having worn the lenses 33 hours (in fact, 12 hours longer than in 1896), Stratton writes that "it is difficult to describe my *attitude of mind toward the inverted scene.* Little more can be said than that there was clearly an abnormal relation between the general localization of my body and the position of the scene as a whole. Once as I stood before the fire-place, watching the fire, an odd sensation came over me, as if I were looking at the fire out of the *back of my head*" (**12**, pp. 349–50). This report, given after he had worn the lenses for a period almost 50 per cent greater than that of 1896, furnishes fairly convincing evidence that Stratton was having no easy time at this stage of the experiment. It also reveals in part the nature of his descriptions. Instead of attempting to describe what he actually perceived or observed in the environmental field, Stratton was unnecessarily baffled by the task of describing his 'attitude of mind' toward the inverted scene.

During the fourth day he writes that "The feeling of the inversion or uprightness of things was found to vary considerably with the strength and character of the representation of my body. When I looked at my legs and arms, then what I saw seemed rather upright than inverted. If I looked away from my body and gave exclusive force to its pre-experimental image, then everything in sight seemed upside down" (**12**, p. 354). In other words, if Stratton considered his body as being upside down, then objects 'seemed *rather* upright.' Scattered among the observational reports of this study are statements from which we may indirectly secure some degree of understanding concerning the psychological basis of the *improvement* that gradually appeared in his ability to manipulate environmental objects. It is quite significant that the foundation of functional change here extended beyond the perceptual level. Significantly enough, increased facility in handling objects was dependent not on touch but on a visual comparison of space and shape properties of objects and an understanding of their relationship. Thus on the fourth day, Stratton reports that "I happened to discover, however, a

simple means of obtaining without calculation the use of the proper hand in picking up things on the floor—*a means which I used thereafter with almost invariable success.* If, with one of my feet near the object, I gave a tap or two on the floor before I stooped to pick it up, the proper hand immediately came into play. Curiously enough, it was easier at this time to start the proper foot than to start the proper hand" (**12**, p. 352).

On the fifth day Stratton was still very far from the well-organized patterns and eye-hand coördinations that some psychologists have referred to as being characteristic of the *third* day. Stratton now reports that "For the first time, I dared to turn and sit down on a chair without beforehand assuring myself with my hands that I had placed myself aright. My movements were of course still cautious and awkward. And often the question of right and left was troublesome; for example, I wished to grasp the handle of the door beside me, and must hesitate a moment before it was clear which hand to use" (**12**, p. 355). The extent of perceptual disturbance at this time is clearly shown in other ways, especially by auditory localization and visual movement. Stratton remarks, that "a friend who was walking with me began to throw some pebbles into the distance to one side of my actual view. The sound of the stones striking the ground came, oddly enough, from the opposite direction from that in which I had seen them pass out of my sight, and from which I involuntarily expected to catch the sound" (**12**, p. 357).

A fuller understanding of the basic foundation of his behavior patterns and the rôle of touch and kinæsthesis in guiding them, may be secured, in addition to what we said concerning the fourth day's observations, from a description written on the *sixth* day: "In putting on my shoes—the lenses were now in place—the problem of right and left, which had *hitherto rendered this operation difficult*, was unreflectingly solved by making a direct visual comparison of the contours of foot and shoe and seeing whether they matched" (**12**, pp. 357–8). On the *sixth* day, we accordingly discover that Stratton is finally able to overcome a difficult task of putting

on his shoes (!) by *comparing* two objects (his foot and shoe) in terms of shape. It would seem entirely logical to assume from his report that he was very far from a functional condition in which he was no longer 'conscious of any difference between his old and new habits' or in which he was able to see and act toward things in a perfectly normal and natural manner.

On the seventh day we find this account showing the continued dominance of vision. "The most striking fact was that the *extent* of the movement now was inappropriate. My hands frequently moved too far or not far enough. . . . When I moved a heated iron with my right hand to that border of the visual field just beyond which, according to pre-experimental localization, my left hand would have been lying, I involuntarily felt an anticipatory shrinking in my unseen left hand . . . although the iron in my right hand was actually several feet from my left, and was moving away from it" (**12**, pp. 464–5).

On the *last* day Stratton's reports apparently indicate a condition of confusion that could be dispelled only through the use of his understanding as applied to the reversal effects in the perceptual field. He reports that "The illusion of contact on the opposite hand to the one actually touched, arose as on the two preceeding days. *I often hesitated which hand was the appropriate one for grasping some object in view, began the movement with the wrong hand and then corrected the mistake.* . . . In eating at table, the movements of my hands and of pieces of food across the visual field, constantly suggested that my mouth must lie between the line of sight and the new position of my legs. But the actual contact on my lips instantly dispelled this suggestion and located my mouth definitely and indubitably on the other side of the line of sight. The place of the actual contact and that of the merely suggested contact were thus in striking contrast" (**12**, pp. 466–8).

Troubled as one may possibly be at times with Stratton's attempts to interpret his mental attitudes, it should be generally evident that his perceptual world, as far as spatial prop-

erties were concerned, *remained at the end as it was in the beginning.* At the end, his reports refer to action patterns which were so awkward and uncertain as to suggest a pathological state under other conditions. The 'illusory' condition still persisted, even with respect to familiar objects that had received considerable practice (fork moving toward head). Stratton's reports imply that man can come to act in accordance with objects, even when such have been shifted widely in his environmental field as a result of a change in their stimulus conditions. But his reports do not show, in any way, that 'experience' finally moulds the perceptual field so that objects are again perceived as they earlier were, *namely*, in terms of former properties. Nor is there the slightest approach to this perceptual condition after 8 days of observation with stimulus 'inversion.' This position finds full sanction in Ewert's studies on this same problem.

Stratton's two reports are quite different with respect to effects arising after the lenses were removed. When the lenses were removed in the 1896 study, "there was no peculiar experience. Normal vision was restored instantaneously and without any disturbance in the natural appearance or position of objects" (**10**, p. 616). This study apparently serves as the basis for the position found in some quarters. The matter is different, however, in the 1897 study. This, apparently, is the basis of the remarks which occur in the other quarters. When the lenses were removed after 87 hours of wearing, Stratton reports, that "the scene had a strange familiarity. The visual arrangement was immediately recognized as the old one of pre-experimental days; yet the reversal of everything from the order to which I had grown accustomed during the past week, gave the scene a surprising, bewildering air which lasted for several hours. It was hardly the feeling, though, that things were *upside down*" (**12**, p. 470). Again, our understanding suffers here from a lack of specific detail. 'Strange familiarity' and 'bewildering air' do not do adequate service. Properties such as size, shape, distance, color are not described. He does, however, mention visual movement. Here, again, we find Stratton's reports in accordance with

those of Ewert. Objects were perceived (1897) in part as in ordinary cases of dizziness and instability. It must be noted that such rapid movement of environmental objects cannot be directly referred to any specific habits established during the wearing of the lenses. Both Stratton and Ewert report the perception of rapid visual movement as soon as the lenses were put on as well as when they were taken off. The former writes that "When I turned my body or my head, objects seemed to sweep before me as if they themselves were suddenly in motion. The 'swinging of the scene,' observed so continuously during the first days of the experiment, had thus returned with great vividness. It rapidly lost this force, however, so that at the end of the hour the motion was decidedly less marked. But it was noticeable the rest of the day, and in a slight degree even the next morning" (**12**, p. 470). It is interesting to note that the most striking feature after removal should be a *perceived characteristic which did not directly concern 'retinal inversion.'* It possibly points toward a serious disturbance of fundamental organic functions involving the use of the ocular muscles. Partial ocular paralysis (some of the eye muscles) is commonly accompanied by the perception of environmental objects in rapid movement as in ordinary cases of dizziness. Ewert points out that when the glasses are put on and taken off the subjects observe the movement of visual objects to be abnormally rapid.

The scientific value of a theoretical position, such as Stratton advanced, rests in part upon its verification by other experimentalists. In 1930 Ewert published the results of an elaborate study upon this problem. It is necessary to evaluate Stratton's results in the light of Ewert's study. Ewert used three subjects over a period about twice as long as that of Stratton (175, 193 and 195 hours). Moreover, his subjects, using binocular vision, worked daily at specific tasks involving perception and action. Extensive quantitative data have been presented upon these tasks. When the full significance of his data is considered, it is evident that Ewert's results do not possess either the touch of 'attentional' magic or the descriptive ambiguity characteristic of Stratton's results. Of his results, Ewert remarks in part that inverted

vision served as a "forceful distraction to normal touch, sound, and kinæsthetic localization, when this behavior is carried on in the field of view" (**5**, p. 353).

When we regard retinal functions in terms of other characteristics of environmental objects than uprightness, we find striking evidence of basic functions. It is accordingly significant that perceptions of visual movement and visual depth clearly reveal themselves to be firmly rooted at a causal level untouched by any 'readjusting process.' Of the former, Ewert significantly remarks that "Directions in movement during disorientation are responded to as though reversed. This behavior cannot be inhibited" (**5**, p. 357). Of the latter, Brown points out that "The evidence is not conclusive that practice with disoriented vision will restore or greatly improve the ability to perceive depth" (**1**, p. 144). Ewert's effects of uninterrupted 'inversion' over a period twice that of Stratton is quite unambiguous. "The illusory visual disorientation effect," he says, "did not change noticeably during a 14 day period of continuous inversion" (**5**, p. 357). Yet during this experimental period, rapid learning went on even under conditions where gross overt localizing movements were, for the most part, absent. This fact is particularly damaging to the theories found in some texts which strongly emphasize the importance of direct overt behavior patterns in the development of perceptual properties. Murphy remarks that Stratton's experiment "shows how in a short time the world may be repatterned." He then continues, "We learn that things are up and down, right and left, far or near, in terms of what we do or would have to do to touch and manipulate these things. Just as the child learns to demarcate the objects of the external world and to find names for them chiefly in terms of the ways in which he can see them, so we learn to build up a world of extensity by actually using such a world" (**8**, p. 189).

Concerning the performances of the subjects following the removal of the lenses, Ewert states that "in most of the experiments the newly acquired behavior during visual inversion *did not persist in a measurable form upon the removal of the lenses*" (**5**, p. 355). Among the experimental tasks was one involving card sorting, a task which demands accurate

localization and effective motor control. His subjects learned (practiced) this problem for 14 days. A task of this sort would appear most peculiarly suited to produce pronounced disturbances immediately following the removal of the lenses. Yet we find Ewert concluding from his experimental data that "Continual practice (14 days) in card-sorting under retinal inversion does not cause interference in the ability to sort cards under normal vision. There is no noticeable persistence of the newly acquired habits upon a return to normal vision" (**5**, p. 320). This specific statement concerning achievement is to be compared with Stratton's vague comments concerning the "strange familiarity" of the perceptual field following removal.

In conclusion we may say that a willingness to accept experimental facts, irrespective of theory, enables psychology to derive an adequate explanation of the contradiction and confusion which exist in the literature upon the specific problem which we have discussed here. It also allows us to secure a fuller understanding of the nature of Stratton's observations. We can thereby distinguish better between his facts and his theoretical interpolations. The actual conditions under which 'uprightness' appears accordingly become clear—they are speculative and not observational. Finally, it enables us to relate Stratton's studies to those of other psychologists upon the same general problem. All such studies clearly show that man may readily establish new action patterns with respect to the visual field even when this has been greatly changed. Such studies reveal the dominance of specific visual field properties over those tactual and kinæsthetic factors which are involved in the new action patterns.

We have sought to show here that the condition of contradiction and confusion to be found in some of our literature concerning space perception is wholly unnecessary and that it has arisen from (1) the duality of Stratton's reports, (2) the uncertainty and ambiguity of his descriptions, (3) the relatively strong emphasis upon mental attitudes, (4) the lack of quantitative data, and (5) the tangle of fact and theory. We have further sought to show that judging from the nature of

his reports, it is evident that Stratton never ceased to be 'conscious of differences' between the old and the new conditions, that he never reached a normal or 'natural' condition of perception or action, and that he never saw objects in an upright position except by ignoring basic structural and functional optical systems and by postulating retinal images as a part of the visual field. A hint of such basic systems is found in connection with the disturbances in movement (and depth) which appear under these experimental conditions. Any doubt concerning the nature of man's observations (with lenses) must be dispelled by an examination of Ewert's data. They show that fourteen days of continuous wearing brought no change in the basic perceptual properties of the visual field. A proper evaluation of Stratton's and Ewert's reports will leave no doubt as to which offers a more solid foundation for theories of space perception.

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A NOTE CONCERNING 'THE NATURE OF DISCRIMINATION LEARNING IN ANIMALS'

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Spence in a recent article in this journal [1] proposes a theoretical mechanism for the sequence of the behavior-acts observed when an animal learns a discrimination problem. In his presentation Spence is particularly concerned with the data obtained by myself and others relative to the pre-solution period of such learning, and he thus has occasion to make frequent references to my data and to my interpretations. Feeling that in several instances Spence has misinterpreted my position and the nature of my problem, and in some cases has ascribed to my interpretation wholly gratuitous 'deductions,' it would be of some value, perhaps, for a clearer understanding of the question involved if I attempted a few comments on his paper.

In the first place I should like to point out that as far as Spence's original contribution is concerned there need not be any point of conflict between what he has to offer and anything I may have said in my previous papers. This does not mean that I necessarily accept Spence's proposed theoretical 'explanation' of my data, but simply that we are really talking about two different things. I had contented myself with an attempt at a description of the learning process on a given behavioral level. In this description I used such terms as 'purposive,' 'docile,' 'attempted solutions,' etc. All of these terms were defined objectively, and there I stopped. Spence, accepting my data, attempts to push the analysis further and show how a certain set of concepts could be used to 'deduce' such 'purposive,' 'docile' behavior-acts. But even if we assume his ability to make such deduction, still

[1] K. W. Spence, The nature of discrimination learning in animals, PSYCHOL. REV., 1936, 43, 427–449.

we cannot hold that therefore the animal is no longer a 'purposive,' 'docile' organism—since these terms were never given a supernatural or inexplicable meaning. Spence has merely attempted a definition in other (and he believes in more basic) terms. I believe that it is the failure of Spence to realize the limitations of what he was doing, and the failure to recognize the difference between our procedures, which is responsible for his 'objections' to my interpretations. Actually Spence does, at one point in his discussion, seem to realize the difference between our procedures. Thus he quotes from an article by Tolman and myself:

> Finally, in closing, it may be pointed out that this descriptive problem of the behavior of means-end-readiness and hypothesis in learning and unlearning is but one phase of the total problem as to all causal conditions underlying the determination and appearance of means-end-readiness and hypotheses. What, for instance, brings about the initial appearance of certain subordinate hypotheses rather than others, and what are the causes of the alternations between one subordinate hypothesis and another.[2]

After that quotation which specifically states that we have not attempted a theoretical account of *why* or *how* hypotheses occur, and after setting as *his own* problem an attempt at such a theoretical picture, Spence proceeds to compare his elaborated 'conditioned-response' hypothesis with an 'insight' hypothesis which I have supposedly put forth!

In the second place Spence seems to neglect consideration of certain important published papers of mine which bear specifically upon points of 'issue,' and then 'criticizes' my interpretation.

I propose, in the remaining space, to take up his objections and criticisms *seriatim*.

(1)

> Krechevsky . . . showed that the reactions of the animal during the pre-solution period were not a chance affair but consisted of systematic modes of response. . . . Furthermore

[2] *Ibid.*, 443–444; quoted from E. C. Tolman and I. Krechevsky, PSYCHOL. REV., 1933, 40, 60–70.

> he concluded that these experimental facts offer conclusive evidence of the inadequacy of the 'trial-and-error' theories of learning, because, as he claimed, they contradict the assumption made by the latter that the learning process is, in its early stages, random and haphazard in nature.[3]

Then Spence proceeds to point out that such an interpretation is invalid since trial-and-error theories do not necessarily involve this 'random and haphazard' assumption. He writes:

> . . . a sophisticated 'trial-and-error' theory of learning would not hold that the order of sequence of trial acts is a haphazard affair, but rather would conceive it as proceeding according to definite principles or laws.[4]

But I never have argued that my data disproved a 'sophisticated' trial-and-error theory. Actually, quite the opposite is true, for in the very same paper from which Spence quoted Tolman and myself, we proceeded to set up and defend a 'sophisticated' trial-and-error theory *based in part on my 'hypothesis' analysis!* Summing up our discussion in that paper we concluded:

> Trial-and-error 'learning' may be defined as the reduction of a means-end-readiness to one of its more specific sub-forms (hypothesis). The laws of such trial-and-error learning have previously been suggested by Tolman as those of sign-gestalt formation.[5]

What I did argue against, and apparently Spence has no fault to find with that, is a trial-and-error theory which did not take into account the systematic nature of the animal's behavior from the very beginning, *i.e.*, a trial-and-error theory which leaned at all heavily upon 'chance' and 'random' responses.

(2)

> In his series of experiments . . . Krechevsky introduced and made considerable use of an insoluble problem, in which no

[3] *Ibid.*, 428.

[4] *Ibid.*

[5] E. C. Tolman and I. Krechevsky, Means-end-readiness and hypothesis—a contribution to comparative psychology, PSYCHOL. REV., 1933, 40, 69.

> one stimulus . . . was regularly the correct one. The development of a systematic mode of response in such a situation was interpreted by him as evidence that the animal, in part at least, must initiate or originate responses and that they are not simply forced upon it 'ab extra.' . . . While one must grant that something within the organism is differentiated . . . it is not at all necessary to ascribe to the animal the powers that Krechevsky's interpretation suggests. It can be easily demonstrated that the theoretical structure developed above [Spence's] would lead to the expectation that an animal would adopt systematic modes of response in such an unsolvable situation . . . the large majority of these hypotheses are positional . . . which suggests that the excitatory strengths of one of the position stimuli, *i.e.*, S_{rb} or S_{lb}, is (*sic*) stronger than the other at the beginning of the experiment.[6]

But what are these extraordinary 'powers' which I have presumably ascribed to the rat, and how does my interpretation differ at all from Spence's? I have merely said, in more 'naive' terms, that for causes which are peculiar to the neurology of the rat (and not to the structure of the apparatus alone) "the excitatory strengths of one of the stimuli is stronger than the other at the beginning of the experiment"! That is all I could possibly have meant by the 'originating' power of the rat, unless, again, one defines 'originating' in supernatural and non-operational terminology. I quite specifically stated, in my original paper, that what the animal will do is a function of his past experience and the peculiarities of his nervous system, and not entirely of the experience he has had now and here with the given specific apparatus.[7] Thus an animal might, when first introduced into a discrimination apparatus, immediately adopt a 'dark-going' hypothesis. This hypothesis may very well be a transfer from some previous experience and not from the training received in the present apparatus. But that is really Spence's view, too, for what else can he mean when he postulates that 'at the be-

[6] *Op. cit.*, 442–443.

[7] I. Krechevsky, The genesis of 'hypothesis' in rats, *Univ. Calif. Publ. in Psychol.*, 1932, 6, 57.

ginning of the experiment' one stimulus has greater excitatory strength than another?

However, Spence asserts, even without "the advantage of such assumed, initial differences between the strengths of the various stimuli, it can be shown that such systematic modes would still be expected."[8] Such a statement, I agree, does contradict my position, but in the first place the existence of this initial difference is not disproved merely because it is not essential; and, in the second place, such a statement does not take into account some very important data which I have already presented. In my paper on the 'Hereditary Nature of Hypotheses'[9] I have demonstrated that, in some cases at least, an initial hypothesis-preference (or, to use more 'sophisticated' terminology, 'a difference between the excitatory strengths of the stimuli') is *demanded.* In the studies reported in that paper it was shown that the mode of hypotheses preferred by the animal (*i.e.*, visual or spatial) was largely determined by the heredity of the animal. This was demonstrated by using two different races of rats which had been selectively bred for seven generations for maze-learning ability.[10] The maze-bright animals consistently adopted spatial hypotheses and the maze-dull animals, visual hypotheses. In the case of these animals, certainly, the adoption of an hypothesis was very much a function of factors within the skin of the animals, factors which were not primarily determined by the apparatus and the stimulus set-up.

(3)

> In interpreting the results of his studies . . . Krechevsky described the behavior of the rat during the pre-solution peroid as consisting of a series of "hypotheses." This name was chosen, he explains, because it suggested four behavioral characteristics describable by the terms (1) systematic, (2) docile, (3) selective, (4) self-initiated. Now obviously such terms merely provide a description and name for the phenomena.

[8] *Op. cit.*, 443.

[9] I. Krechevsky, Hereditary nature of hypotheses, *J. Comp. Psychol.*, 1933, 16, 99–116.

[10] R. C. Tryon, Individual differences in the final learned act, in 'Proceedings, Western Psychological Association,' *Psychol. Bull.*, 1932, 29, 545–546.

> . . . That Krechevsky himself, recognizes this is indicated by the following quotation from a joint publication by himself and Tolman. . . . "Thus . . . all such terms as those of 'desire' . . . 'expectation' . . . 'hypotheses' are merely names, or concepts, for relationships between variables. They do not say anything about the *vera causa* of these relations."

Then, Spence admits, "One could certainly have no quarrel with this point of view." [11]

After assuring the reader that one could have no quarrel with such a point of view, Spence immediately, in the same paragraph, proceeds to quarrel, and accuses me of thinking of hypotheses as 'purposive attempts at solution.' [12] But that is exactly what I do mean and have meant by 'docile,' 'selective' and 'attempted solutions.' Those behavior characteristics *are* the defining criteria of 'purposive' attempted solutions. If he has no quarrel with my using the descriptive term 'docile,' he cannot possibly have any quarrel with my using the word 'docile' when spelled 'purposive'—I am still using the word as operationally defined. I have not attempted to sneak in an albino soul by the use of that word. Spence here can only object to my diction, not to any meaningful concept which I am employing—*a concept the mechanism of which he is attempting to describe.*

(4)

Spence maintains that according to my conception of 'hypothesis,' several deductions may be drawn, one of which is

[11] K. W. Spence, The nature of discrimination learning in animals, PSYCHOL. REV., 1936, 43, 443–444.

[12] Throughout Spence's article, whenever he has occasion to use the phrase 'attempted' solutions, he very carefully encloses it in quotation marks as if to disclaim any responsibility for such unscientific terminology. It is interesting to note that in an earlier article by himself in which he made an analysis of the behavior of the chimpanzee in the pre-solution period of learning, Spence concluded (without the *cordon sanitaire* of quotation marks) ". . . almost from the first the chimpanzee adopts one or another of the possible relational responses . . . and it continues to *try out* different ones until it hits upon the correct one. . . . The sequence of selection of *attempted solutions* appear to follow a definite pattern. . . ." (Italics my own.) See Spence, Systematic versus random responses in the solution of multiple choice problems by the chimpanzee, in 'Proceedings, American Psychological Association,' *Psychol. Bull.*, 1934, 31, 688.

> . . . that if the values of the cue stimuli are reversed, *i.e.*, the positive stimulus made negative and vice versa, *before* the animal begins to show any learning whatever, it should not necessarily make for any slower learning of the reversed problem, for, according to this theory the animal selects and concentrates, in turn, on certain aspects of the experimental situation . . . and does not react to the real cue aspect until just preceding the time of solution. . . . The implication of our own [Spence's] hypothesis . . . on the other hand, is quite definitely opposite in nature.[13]

Spence then cites some evidence from McCulloch and Pratt which he interprets as favoring his 'own' view. But had Spence looked more carefully, he could have obtained just as conclusive evidence for his 'own' view in one of my papers. I have explicitly stated, in several of my articles, that very frequently the animals seem to be responding to more than one hypothesis at the same time:

> These two kinds of responses, spatial and visual, overlap in many cases. For example . . . every time the animal departed from his dark-going habit, he did so only to enter an alley on the right side of the box. . . . In other words, it may be that *on any one day the animal may have been responding not on the basis of one habit but two habits, a visual and a spatial one.*[14]

An animal *may* (and in many cases probably does) select only one cue out of the stimulus field, and in such cases a change in the value of the cue stimulus may be without effect on the learning speed; but he may also (and that I have demonstrated) before dropping one hypothesis entirely, or to any great extent, select another cue and continue to divide his responses between the two. In this latter case, obviously, a reversal of the meaning of the second cue (if it happened to be the correct one) would interfere with the learning speed of the animal. Nothing that I have said rules out either of the two possibilities—actually from the data I have presented, *both* of those possibilities would be expected.

[13] *Ibid.*, 444.

[14] I. Krechevsky, The genesis of 'hypothesis' in rats, *Univ. Calif. Publ. in Psychol.*, 1932, 6, 58.

In concluding I wish merely to say a word on words. There can be no possible objection, scientifically, to the use of *any* word; the objection can merely be to its definition. If I am interested in behavior which the man in the street calls 'purposive,' I can study that behavior scientifically and even call it 'purposive behavior' *so long as I define all my terms operationally*. Redefining the same behavior in any other acceptable set of terms does not in the least deny my definition, nor add nor subtract anything to or from it.

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